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PHOTOGRAPHIC OBSERVATIONS OF A VERY REMARKABLE COMET.

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FOR POPULAR ASTRONOMY.

Daniel's comet of last year, 1907 d, though it had apparently every advantage in its favor—a relatively near approach to the Sun and a naked eye visibility that extended over at least two months, a splendid object in reality—was a disappointment in that it did not give us any new phenomena. The changes in the tail—which was long and splendid—were relatively few, and when the comet finally reached perihelion it had settled down to a steady flow of tail producing particles with essentially no changes. Altogether it seemed to be a well-behaved body, without any desire for spectacular display.

Morehouse's comet of this year 1908 c, though it has remained a relatively faint object in the telescope, has supplied us with all that was wanting in the comet of last year in the way of new phenomena. It is certainly the most bizarre comet that we have had to deal with since photography began to register the freaks of the tails of comets. Almost nightly it has shown features that would have singled it out as a very remarkable object, and on more than one occasion it has presented a most extraordinary and unique appearance.

The photographs would seem to show that the light of this object is much more strongly photographic than that of Daniel's comet, for though very much fainter, it really photographs quicker than the brighter one. I have secured photographs of it on every possible occasion, and on several nights when the opportunity occurred, I have photographed it throughout the night, changing the plates at intervals of one or two hours.

Altogether up to and including November 3, I have made 239 photographs of the comet on 47 nights, with the 10, the 6¼ inch and the 3.4 inch lenses.

10 inch	79
6¼ "	91
3.4 "	69
Total	239

1908 October 3. 17h 7m G. M. T. Exposure 2h 25m. 10-inch Bruce Comet.

The result so far is the best collection of photographs of a comet yet obtained, and the accumulation of material that must be of the highest importance in the solution of the mysteries of comets—even if it does prove revolutionary in some of its results. It is too early to digest this material, especially as the comet is still under observation and doubtless will give us yet more valuable information.

Daniel's comet was visible only for a short time in the mornings and permitted but a comparatively short observation before daylight. The duration of its visibility above the horizon when bright, was too short to allow much change to be shown by successive exposures. It was necessary therefore to compare photographs made at different observatories on the same night to show the changes, which, to begin with, were neither striking nor great.

The present comet was placed at a high declination (+76° part of the time) and visible through the entire night. A splendid opportunity was thus offered to detect changes in it at the same observatory by frequent photographs during the night. Added to this fact was the still more important one that the comet, much of the time, was changing rapidly and sometimes violently.

During part of September the atmosphere was filled with dense haze and smoke, so that for some days the Sun itself could scarcely shine through with a feeble yellow light. At night only the brighter stars could be seen. Under these conditions it was very difficult to get anything in the way of a photograph of the comet, and then only with long exposures. The guiding was also specially difficult because of the faintness of the comet.

There have been several periods of remarkable outbursts in the comet. Between these displays it has not been lacking in interest, for there is essentially not a single picture that is not more or less important. The principal disturbances, which are covered by my plates, occurred on September 30 and October 15. There was doubtless one also on or about October 6, the main part of which was lost here in moonlight.

The first picture of September 30 scarcely suggested what sort of an aspect the comet would present before the night was over. It showed it different from its previous appearance, but not enough so to cause special remark. The succeeding photographs on that night, however, became more startling, the last one being extremely remarkable and beautiful. The tail had become cyclonic in form and was attached to the head (which was

small and almost star-like) by a very slender curved tapering neck which was all but detached, and must within an hour or so have become completely separated from the head. That this separation took place was evident from the photographs of the succeeding night, where the wreck of the tail was shown as a very large long mass some two degrees from the comet, and apparently attached to it by one or two slender threads or streams of matter. This mass which had formed the tail on September 30, was visible on the photographs for several days, farther away and fainter until it finally dissipated into space. The next large outbreak occurred on October 15 when the photographs showed first, that the tail for one half degree out was made up of a slender stream which joined on to great cloudlike masses which were nearly at right angles to the tail. From these latter a broadening tail, curved and irregular on its north side ran eastward for six or eight degrees. Successive photographs at an hour's interval showed that, as usual, these masses were moving away from the comet. They seemed also to have a lateral motion in the direction of the comet's flight, and apparently moved faster southwards than the comet itself. From these condensations, slender tails or streams extended for a degree or more along the direction of the main tail. These same masses are seen further out on the plates of Oct. 16 and 17.

Evidently a similar disturbance occurred on the 6th of October, for the tail presented a like appearance on that date—a straight narrow tail for one degree which joined on to great masses like clouds of smoke. These were still faintly seen on the plate of October 7.

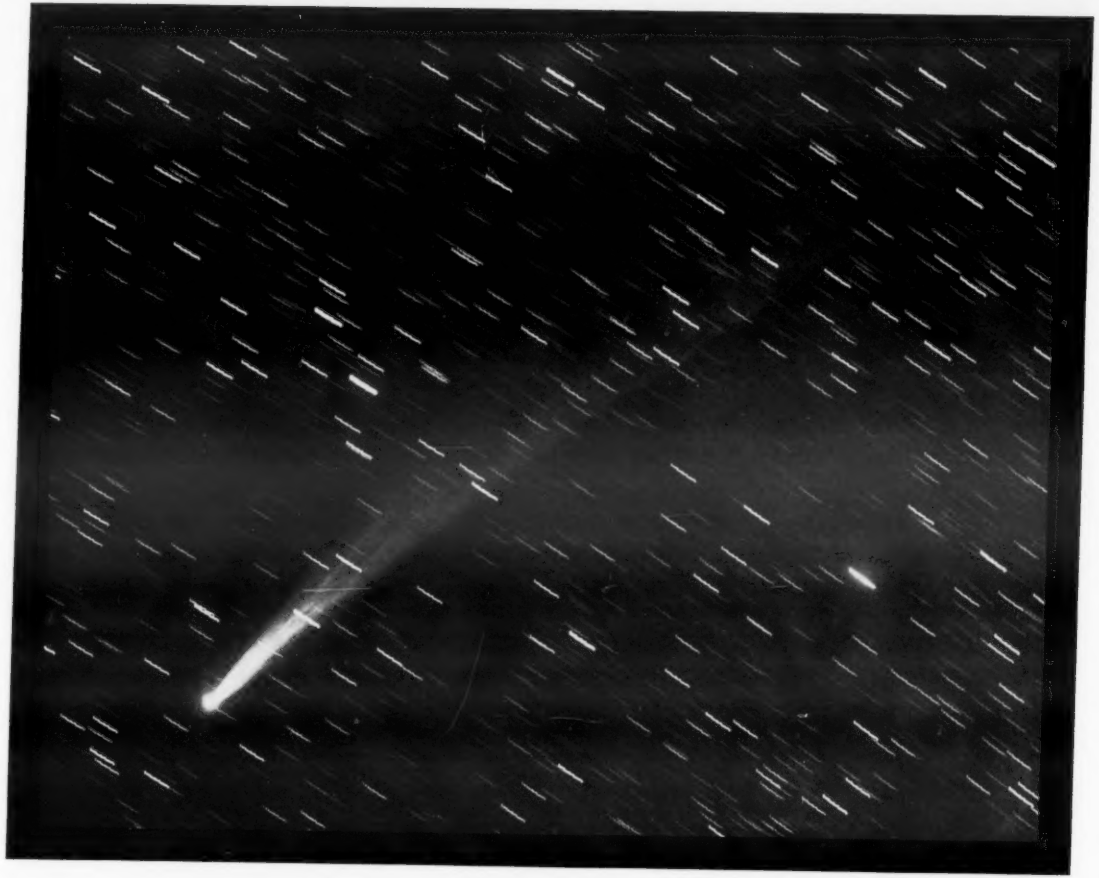
There has been much deviation in the brightness of the comet from its theoretical value. At times it has been much brighter not only photographically, but visually also. On October 14 and 15, I could see it very faintly with the naked eye as a smudge of light with a suggestion of tail for some 3° or 4°. On the 17th with similar conditions I could not see it. On the 21st, it was again faintly visible. October 28 it was seen with the naked eye as a faintish mass with a faint streak of tail for several degrees. On the 29th it was quite conspicuous to the naked eye. By averted vision, the tail could be traced with the eye to about 2° north of β Cygni, or for about 5° or 6°. It was quite noticeable before the Moon set and afterwards would readily have been seen by any one glancing in its direction. It was very much brighter than on the 28th. Later on it seemed to be fainter to the naked eye—more so I think than would be ac-

counted for by the lower altitude.

On October 30 the comet was again faint to the eye, much less bright than on the 29th, though it could be seen as a faint hazy spot with a faint streak from it. The comet, therefore, suddenly brightened up sometime between October 28 and 29 and faded out again by the 30th. On October 29 the head seemed to become smaller in the guiding telescope and the part of the tail visible in the field became narrower.

I have several times examined the comet with the 40-inch, but these views, from the contracted field and the great power of the instrument, have been unsatisfactory. On September 20 I could see that the tail near the head was almost disconnected and that it was receding from the comet. This brighter portion of the tail was several minutes long. At $8^h\ 13^m$ (C.S.T.) the center of mass was 7′.0 south of the head. "It is quite noticeable, and is about 1¼′ wide by about 3′.5 long." At $11^h\ 33^m$ the mass had moved away from the comet and was 11′ distant. It had changed much in form. The apparent rate of recession was 1′.2 per hour in this interval. A photograph which I made on the same night showed that this object was the disconnected end of a considerable tail and that a fainter stream connected it with the head. On October 11 a minute nucleus of 14^m or 15^m, like a faint star, was visible following the central brightness of the head by about 5″. As the comet was moving rapidly southwards and I was measuring it for position, there was no question as to this being the nucleus. On account of its unsymmetrical position, I at first supposed it was a faint star.

The early observers of comets noticed the fact that the tails of these bodies were always directed away from the Sun. It was naturally concluded that this peculiarity was due to a repulsive force existing in the Sun, which drove the smaller particles of the comet off into space. This conclusion was readily reached but the force itself was unrecognized. Modern astronomy, through the researches of Arrhenius and the experiments of Nichols and Hull, has shown that the repulsive force is really the pressure of the Sun's light upon the smaller particles of the comet. If the particle is so small that its surface is relatively large compared with its mass, then the sunlight pressing against it will overcome the pull of gravity and drive the particles away into space. The smaller the particles the greater will be their velocity. From this it will be seen that when the conditions are right the smaller particles will be sifted out from the



NORTH

COMET 1908 *c* (Morehouse)

1908 October 4, 19h 5m G. M. T. Exposure 1h 45m; 10-inch Lens, Bruce Telescope, Yerkes Observatory

more massive ones and will flow away from the comet, thus forming the tail, which will point away from the Sun. At present this theory is the most satisfactory one yet offered to account for a comet's tail and has been generally accepted. It is probable that it may need modification to suit special cases in different comets.

Photographs of comets in recent years have shown such rapid changes in the forms of the tails of these bodies, that we cannot accept the pressure theory as accounting for all the peculiarities of the tail. It doubtless is the *cause* of the tail but there must be other influences at work—possibly electrical. I have been forcibly struck with the resemblance of some of the phenomena of the aurora to some of those of the comet—especially in the sudden brightening up of parts of the tail where apparently no material existed before and where there is no visible supply reaching it from the comet. Such peculiarities have occurred in the present comet and were specially noticeable in the visual observations of Sawerthal's comet in May, 1888. (*Astro. Nach.* 2859, Bd. 120, p 43). The abrupt change of direction of the tail and the sudden transformation from a straight to a violently curved tail demand some other explanation than simply the pressure theory. Doubtless the tail is produced by light pressure, but some other agency is responsible for its distortions, deflections, etc.

The large receding masses that are shown sometimes on the photographs of comets can be accounted for, in a way, by the assumption that they are masses of denser particles that are thrown off by the comet under the disturbing influence of the Sun, and which, as a whole, are driven away by the light-pressure. Because of the large size of most of the particles composing these masses their motion is relatively slow, very much slower than the particles that form the general tail. That this is so, is manifest from the fact that streams or secondary tails have been shown, in several cases, issuing from them like the main tail from the comet. These secondary tails, or streams are apparently due to smaller particles that are mixed with the larger ones of the masses, being sifted out and driven away far more rapidly by the light-pressure. Examples of these secondary tails are shown in photographs of Swift's comet on April 7, 1892, (Popular Astronomy, Vol. 12, p. 2, 1904) and in Morehouse's comet on Oct. 15.

Two photographs of the comet are given here. They were both made with the 10-inch Brashear lens of the Bruce teles-

cope of the Yerkes Observatory. They show how rapidly the comet changed its character. In that of October 3 the tail is made up of a number of separate rays. These rays in the last photograph of that night were diffusing together, doubtless to form the tail shown in the second picture—October 4th. In this last one (Oct. 4) the head was very small—smaller than is shown in the reproduction; the tail was curved and irregular on its northern side.

There is no resemblance between these two photographs of the same object on these two successive nights. Yet the transformations were so great and rapid that on September 30 there was almost no resemblance between the comet in the first picture and in the last one of that night—an interval of less than four hours. The tail has sometimes been perfectly straight and at others violently curved. Indeed this comet could readily produce all of Bredikkine's three types of tail in as many days or less. These pictures have shown how unsafe it is to say that a comet was of a certain class or type from any one picture of it. It is not at all likely that the nature of the particles has changed in producing the various tails shown by this comet.

Yerkes Observatory,
October, 1908.

ON AN INFINITE UNIVERSE.

W. H. S. MONCK.

For Popular Astronomy.

That the limited quantity of light which we receive from the stars does not prove the universe to be limited even on the hypothesis that no light is lost in transmission may, I think, be conceded. But though we may make suppositions which would reconcile the actual facts with an infinite universe, the question arises whether these suppositions are not of such an improbable character as to render a finite universe almost certain. Thus if the stars became more thinly scattered the farther we proceeded in every direction from the Sun taken as a center, the number of stars might be infinite and yet the total amount of light very limited. But if there is no reason to regard the Sun as situated in the center of the universe and if we regard it as a comparatively insignificant star which has but little influence on the motions of the other bodies which surround it, the hypothesis that the Sun is situated in the densest portion of the universe

and that the stars become more and more widely separated as we move away from it in any direction seems a very improbable one. The more natural supposition is, I think, that of uniform distribution—by which I mean *average* uniformity. As we pass outward from the Sun, the stars may become thicker in one direction and thinner in another, but if we carry the process far enough, the excess in one direction will balance the defect in another, and probably even in the same direction the excess of stars at one distance will be balanced by the defect at another. If the solar system is too insignificant to affect materially the positions of the visible stars, average uniformity is, I think, the most reasonable supposition that we can make. I only assume it provisionally however. Without assuming some law of distribution numerical results are unattainable, and this seems to be the most natural assumption.

Star-magnitudes are best measured by the scale suggested by Mr. Pogson and adopted in the Harvard Photometries. According to this scale a star of nth magnitude is brighter than a star of the $n+1$th magnitude in the proportion of $10^{0.4}$ (2.512) to 1. As intensity of the light varies inversely as the square of the distance, on the hypothesis of uniformity the average distances of the stars would be in the ratio of $10^{0.4}$ to 1, and by comparing the successive spherical shells formed by describing spheres with radii thus related to each other round the Sun as center we arrive at the proportion $10^{0.6}$ to 1 or very nearly 4 to 1 for the relative number of stars of successive magnitudes. The hypothesis of uniform distribution thus gives (if we adopt Pogson's scale) 4 times as many stars of the $n+1$th magnitude as of the nth magnitude. But the star of the nth magnitude is on the average 2.5 times as bright as the star of the $n+1$th. The total light of the fainter stars is thus greater than the total light of the stars one magnitude brighter in the ratio of nearly 4 to 2½ or 8 to 5. But if the total light of the $n+1$th magnitude is greater than that of the stars of the nth magnitude, the total sky-light (on the hypothesis of no loss of light in transmission) would be infinite. It is easy to see, however, that the hypothesis of no loss of light in transmission is one that cannot be true of such an infinite universe as I have been considering; for the stars would ultimately get in each other's way and intercept each other's light. The greatest imaginable quantity of light would be given when the whole sky was covered by star-disks whose light would be comparable—I do not say equal—in intensity to that of the

Sun. But if there are (as I presume there are) dark bodies in space, a considerable portion of the sky may be covered by black disks instead of bright ones and the general illumination of the sky may fall considerably short of that of the Sun's disk owing to these black spaces. Both the bright and the black disks would probably be too small to be seen as separate objects, and the presence of the black disks would be only indicated by a general reduction in the luminosity of the sky as compared with that of the Sun.

I have compared the numbers of stars of each magnitude in some of the Photometric Catalogues in which Pogson's scale has been adopted; and the general result has been that while the theoretic *ratio* of 4 to 1 is never attained, the ratio of 2½ to 1 is always exceeded, so that the total light of the stars of the stars of the $n + 1$th magnitude is always greater than the total light of those of the nth magnitude. But these catalogues only take us as far the 8th magnitude (perhaps not so far) and from the comparatively small amount of the total star-light, I think this state of things must be inverted before we go much further and that very possibly the total light of the stars of the tenth magnitude exceeds the total light of the stars of the eleventh magnitude. Assuming this to be the case, however, the question remains whether this falling off in the total light of the fainter stars is due to a thinning out of the stars when we reach the average distance of a tenth-magnitude star or to the loss of a considerable portion of the light of the more distant stars in transmission. The latter seems to me to be more probable.

Assuming that there is no loss of light in transmission the intrinsic brightness of a star-disk will be the same at all distances. For the light varies inversely as the square of the distance and so does the angular magnitude of the disk, so that the dimensions of the visible object are reduced in exactly the same proportion as its total light, In the case of the stars what we see is not the real disks but a spurious disk caused by the imperfection of our eyes or our instruments; but I think there can be no doubt that this defect does not reduce the quantity of light which we perceive while it evidently cannot increase it. The total amount of light which we receive from the sky is therefore the same that we would receive if all the star-disks were seen in their real dimensions with their real intrinsic brilliancy: and from this we may draw conclusions as to what proportion of the visible sky is covered by luminous star-disks.

What would be the angular dimensions of a disk having the same luminosity as the Sun and giving as much light as all the fixed stars put together? The answer on the *data* given by others as well as Professor Hough is rather a startling one. I doubt if the diameter of the disk would amount to one second. I do not say that it is impossible that the disks of an infinite number of luminous stars could be collected into so small a space as this, but I think it is highly improbable. From what we know of the nearer stars I would say that the stars which make up an infinite universe must be distributed in a very strange manner in order to bring about such a result.

The facts appear to me to be much more easily explained by the assumption of a loss of light in traversing space. I think it very probable that the ether absorbs light and heat—perhaps changing its nature as it does so and becoming a fine nebula.* But dark bodies in space seem sufficient to account for the loss. Besides the larger dark bodies revealed by spectroscopic researches it is probable that there are countless myriads of smaller ones from the asteroids of which numbers are detected every year to the meteors moving around us millions of which enter the atmosphere every day. These may be regarded as cosmic dust, and though their effect may be imperceptible when light has only 100 millions of miles to travel it may be considerable when this distance is multiplied by a million as it would be even in the case of a comparatively near fixed star. A haze which hardly affects our vision of objects close at hand may entirely shut out a distant mountain.

I cannot believe that there is any distance which would render a luminous object absolutely imperceptible—provided, of course, that the ether extends to that distance. The object sets the ether in vibration. If the ether does not absorb that vibration and it is not absorbed by some intervening body, it must reach us though perhaps in a very enfeebled form; and that enfeebled vibration if sufficiently magnified (by resonance, by combining with other feeble vibrations, etc.) will ultimately make its impression on the sensitive plate or on the eye. There is no abrupt passage from consciousness to unconsciousness. Stars which are separately invisible effect the eye when placed close together and the same thing is true of the telescope and the photograph in which latter case a long exposure enables us to see what a short-

* On this hypothesis the great outburst of light and heat involved in a New Star may *create* a nebula.

er exposure fails to indicate. What fails to excite consciousness may still be active, and may with a little more activity of the same kind be rendered perceptible. Sight is peculiarly circumstanced in this respect. We receive the star-light in the daytime but do not see the stars. We fail to see a good many of them on a moonlight night, and on a very dark night if the sky is clear, we can see stars that are not usually visible. We might not impossibly see a star of the tenth magnitude with the naked eye if all other light could be excluded. Contrast of light is an important element as regards the visibility or invisibility of any object. A small star may be lost in the glare of a large one though it would be seen easily enough if the large star were removed. Light which is insufficient to produce conscious perception may nevertheless by affecting the organ of vision render it more susceptible to similar influences than it would otherwise; and the organ when under the influence of a strong light will not be perceptibly affected by minor lights which under more favorable circumstances would be easily perceived. Assuming the ether to be completely transparent, can we believe that the entire sky might be covered with luminous star-disks and yet appear black to us because each disk taken separately was too small to be seen? The Sun's luminous surface consists of an assignable number of square inches. One square inch of it would, I presume, be invisible to a spectator on the Earth if all the rest were shut out by an eclipse; but will it be contended that the Sun's surface cannot be seen because it is composed of luminous parts too small for separate vision?

The argument in favor of a finite universe does not turn on the total light of the sky being finite but on its being very small—too small to be consistent with an infinite universe unless we make some rather extravagant supposition with regard to star-distribution. Supposing that $\frac{1}{500{,}000}$ part of the sky were covered with star-disks having an average brilliancy equal to that of the Sun and that these star-disks were distributed impartially over the sky, the entire sky ought to be as bright as the full Moon. Is this degree of brightness attained even in the densest star-cluster? If the number of stars be infinite, it seems strange that on drawing a line at random through space the chances are at least 500,000 to 1 that it will never encounter a star although produced to infinity.

But is there any mode of testing the current hypothesis that no light is lost in transmission through the ether? I think there is.

The average distance corresponding to a decline of one magnitude is, as I have remarked, $10^{0.2}$ to 1. Suppose now that we ascertained the distances of a number of representative stars of the nth and also of the $n + 1$th magnitude and found that the average distance of the fainter stars was in less than this ratio to that of the brighter ones what inference could we draw from this? Clearly that the light of the stars at increasing distances diminished more rapidly than in the ratio of the inverse square of the distance—in other words that some light was lost in transmission. It will be long before we can know accurately the distances of a sufficient number of stars to apply this test. But we may be able to reach an average result indirectly. The proper motions in R.A. and Decl. of stars with similar spectra may be assumed to be (on a general average) inversely proportioned to their distances from us, and in this way the relative distances of stars of the nth and $n + 1$th magnitudes with similar spectra may inferred. If the distances give $10^{0.2}$ to 1 within the limits of probable error the medium would seem to be transparent, but if the ratio is sensibly less than this there would appear to be a loss of light in transmission. More than one writer has noticed that faint stars often have larger proper motions than we should expect from their faintness. If this be true on a general average of the fainter stars, it would afford a strong argument in favor of the loss of light in transmission. And such a loss of light in transmission affords, I think, the simplest explanation of the fact which I noticed that the stars of the $n + 1$th magnitude are never four times as numerous as that of the nth magnitude their theoretic ratio on the hypothesis of uniform distribution and no loss in transmission. It seems to me certain, however, that there must be a loss of light in transmission even if the ether itself is non-absorbent. The question is rather whether this loss may be large enough to account for the facts which have been relied on as proving the finiteness of the universe or whether the total absorption would not be insignificant even in the case of the most remote stars that can be seen or photographed by our present methods. There is here an interesting field for future research which I hope will not be permitted to lie fallow.

Dublin, Ireland.

RIGIDITY OF THE EARTH.*

T. J. J. SEE.

Rigidity of the Earth Calculated from the Theory of Gravity, on the Hypothesis that the Distribution of Rigidity in the Globe is Everywhere Proportional to the Pressure.—It has not been supposed by previous investigators that a method could be devised for deducing the rigidity of a body like the Earth from the theory of gravity; but in 1905 it occurred to the present writer that such a method could be found if we could adopt a suitable hypothesis for the variation of the rigidity with the pressure. Previous investigations of the internal state of the heavenly bodies had justified the law of Laplace as giving an excellent approximation to the law of density for the Earth and the rest of the encrusted planets; and the monatomic law had been found most satisfactory for the Sun and fixed stars (cf. *A. N.* 4053). These laws enable one to obtain the pressure at every point of the radius of the heavenly bodies. For in several ways Laplace's law of density is fairly well established for the Earth, and on equally good grounds the density of the Sun is believed to conform essentially to the monatomic law.

From a study of the laws of density, pressure and temperature within the heavenly bodies it appeared to me (as it had independently appeared to Arrhenius five years before) that matter under these extreme conditions must be essentially gaseous; and as it is above the critical temperature, it is made to behave in confinement as an elastic solid. Now in all gaseous masses the density is proportional to the pressure so long as the gas remains perfect; and the gas does not cease to be perfect when the temperature is above the critical value, though it it may acquire in confinement the property of an elastic solid if the pressure be great enough to bring the molecules within a distance at which the molecular forces become effective in spite of the high temperature. Thus while the property of rigidity in cold solids depends wholly on molecular forces which prevent deformation, this property for gaseous matter in confinement under such pressure that it acquires the property of an elastic solid, is due wholly to the pressure. The molecular forces giving effective rigidity must increase in proportion to the pressure, or in a higher ratio.

If according to hypothesis the matter is made solid by pres-

* Extract from Dr. See's Physics of the Earth.

sure, then the molecular forces resisting deformation in the imprisoned matter thus solidified cannot resist deformation in a less degree than the direct proportion to the pressure on which the solidification depends. And any ratio higher than the direct proportionality to the pressure would most likely depend on the temperature. Now the temperature in the Earth is supposed to be everywhere such as to make the density conform essentially to Laplace's law; and the pressure resulting from this law of density gives the matter everywhere the property of an elastic solid, and therefore its molecular properties must correspond to the physical state determined by the laws of density and pressure.

It is of course conceivable that some parts of the globe might be relatively more rigid than is required to give solidity, but the effect of this would only increase the average rigidity of the Earth as a whole. And since seismological and other observations seem to show that the globe is solid throughout, except a thin layer just beneath the crust, the hypothesis of a rigidity proportional to the pressure will give a true minimum value of the Earth's rigidity.

Now on the hypothesis that the density follows Laplace's law, the pressure throughout the Earth's mass is given by the formula (cf. *A. N.*, 4104)

$$P = \frac{3x}{2(\sigma_1 g) q^2}[(\sigma g)^2 - (\delta g)^2], \qquad (1)$$

where r is the radius of the Earth, g mean gravity, q the constant for Laplace's law, 2.52896 radians = 144° 53′ 55″.2, σ the density at any point, δ the density at the surface, and σ_1 the mean density.

To render this expression avaible for integration throughout the sphere occupied by the Earth's mass, we must put for σ^2 its value

$$\sigma^2 = \sigma_0^2 \frac{\sin^2 (q\,x)}{q^2\,x^2},$$

and for δ^2 its value

$$\delta^2 = \sigma_0^2 \frac{\sin^2 q}{q^2},$$

corresponding to the surface where $x = 1$. Thus we obtain

$$p = \frac{3(\sigma_0 g)^2\, r}{1(\sigma_1 g)\, q^2}\left[\frac{\sin^2 (qx)}{q^2 x^2} - \frac{\sin^2 q}{q^2}\right]. \qquad (2)$$

For the total pressure throughout a sphere of radius $\rho = rx$, r being the external radius, and $x = \frac{\rho}{r}$ = fraction of the radius, we have

$$P = \int_0^x p \cdot 4\pi r^2 x^2 \cdot r dx \qquad (3)$$
$$= \frac{3(\sigma_0 g)^2 \cdot r 4\pi r^3}{2(\sigma_1 g) q^4} \left(\int_0^x \frac{\sin^2(qx)}{x^2} x^2 dx - \sin^2 q \int_0^x x^2 dx \right),$$

which by integration becomes

$$P = \frac{3(\sigma_0 g)^2 r 4\pi r^3}{2(\sigma_1 g) q^4} \left(qx - \sin(qx) \cos(qx) \over 2q - \sin^2 q \frac{x^3}{3} \right), \qquad (4)$$

As our integration is to include the whole sphere of the Earth, we put $x = 1$, and then we have

$$P = \frac{3(\sigma_0 g)^2 \cdot r 4\pi r^3}{2(\sigma_1 g) q^4} \left(\frac{q - \sin q \cos q}{2q} - \frac{\sin^2 q}{3} \right). \qquad (5)$$

The total volume of the Earth is $(\frac{4}{3})\pi r^3$, and hence the average pressure per unit of area on all concentric spherical surfaces is

$$R = \frac{P}{\frac{4}{3}\pi r^3} = \frac{3}{4\pi r^3} \int_0^x p \cdot 4\pi r^2 x^2 \cdot r dx \qquad (6)$$
$$= \frac{9(\sigma_0 g)^2 \cdot r}{2(\sigma_1 g) q^4} \left(\frac{q - \sin q \cos q}{2q} - \frac{\sin^2 q}{3} \right).$$

If r is expressed in meters, the mean pressure or mean rigidity R comes out in kilograms per square meter. To reduce the result to atmospheres we divide by 10,333. The result for the Earth is $R = 748{,}843$ atmospheres, about the rigidity of wrought iron.

This method takes no account of the Earth's solid crust, and is therefore too small; morever viscosity increases within the Earth, owing to the rise of temperature downward. We give hereafter an approximation to the increase of rigidity by determining the mean rigidity of the Earth's *matter*, as distinguished from that of the *various layers* composing the globe, just found by the above analysis.

To find the mean rigidity of the Earth's *matter* we must consider not only the pressure but also the density or mass per unit volume of the imprisoned matter in each layer. The result represents a mean rigidity in which every elementary spherical shell composing the globe is allowed a weight proportional to its mass, which is multiplied by the pressure to which it is subjected.

The theory of the determination of the mean rigidity of the Earth's matter is as follows:

$$P' = \int^x p \cdot 4\pi r^2 x^2 \cdot r dx \cdot \sigma = 4\pi r^3 \sigma_0 \int_0^x p \cdot x^2 dx \frac{\sin(qx)}{qx}. \qquad (7)$$

Substituting for p its value from (2), we get

$$P' = \frac{3(\sigma_0 g)^2 \cdot r \cdot 4\pi r^3 \sigma_0}{2(\sigma_1 g) q^5} \left(\int_0^x \frac{\sin^3 (qx) x^2 dx}{x^3} - \sin^2 q \int_0^x \frac{x^2 \sin (qx) dx}{x} \right), \quad (8)$$

$$= \frac{3(\sigma_0 g)^2 \cdot r \cdot 4\pi r^3 \cdot \sigma_0}{2(\sigma_1 g) q^5} \left(\int_0^x \frac{\sin^3 (qx) q dx}{qx} - \sin^2 q \int_0^x \frac{x \sin (qx) q dx}{q} \right). \quad (9)$$

The integral of this last term is $- \sin^2 q \, \frac{\sin (qx) - qx \cos (qx)}{q^2}$.

The value of the first integral is most conveniently found by quadrature, table for which is given in *A. N.*, 4104, p. 379. Dividing out the mass, or volume of the sphere by the density, we have

$$R' = \frac{P'}{\frac{4}{3}\pi\sigma_1 r^3} = \frac{3 \int_0^x p \cdot 4\pi r^2 x^2 \cdot r dx \cdot \sigma}{4\pi\sigma_1 r^3} \quad (10)$$

$$= \frac{9(\sigma_0 g)^2 \cdot r \cdot \sigma_0}{2(\sigma_1 g) q^5 \sigma_1} \left(\int_0^x \frac{\sin^3 (qx) q dx}{qx} - \frac{\sin^2 q}{q^2} [\sin (qx) - qx \cos (qx)] \right).$$

On putting $qx = 144^\circ\ 53'\ 55''.2$, the value of the integral is found by quadrature to be 0.9592502, and when the rest of the formula is reduced to numbers we have (*A. N.*, 4104):

$$R' = 1028702 \text{ atmospheres.}$$

The rigidity of nickel steel is taken to be 1,000,000 atmospheres. It thus appears from this calculation that the average rigidity of all the Earth's matter somewhat exceeds that of nickel steel. The actual rigidity of the Earth almost certainly lies between the limits thus established, namely $R = 748{,}843$, based on the rigidity of the layers deduced from the pressure to which they are subjected, and $R' = 1{,}028{,}702$, derived from the product of the mass of each layer by the pressure acting upon it.

In the paper, "Researches on the Rigidity of the Heavenly Bodies," *A. N.*, 4104, the rigidity of the Earth is discussed as follows:

"When one considers the effects of the enclosing crust and the viscosity of the whole Earth, which must be assumed to increase towards the center, owing to the increasing density and rising temperature of the imprisoned matter, it seems not improbable that the actual effective rigidity of our globe may be nearer the upper limit than the lower, and probably we shall not be far wrong in concluding that it is approximately equal to that of nickel steel.

"Leaving aside the consideration of the effects of the solidified crust, it is evident from the nature of the forces at work that most of the yielding of our globe, due to the periodic action of small forces, is in the outer layers; and in general the yielding in

any concentric layer may be taken to be inversely as the pressure to which the imprisoned matter is subjected. *It is remarkable that the curve of pressure as we descend in the Earth becomes therefore also the curve of effective rigidity for the matter of which the Earth is composed.* Thus the rigidity of the matter at the Earth's center probably is at least three times that of nickel steel used in armor plate; as we approach the surface the effective rigidity constantly exceeds that of nickel steel until we come within less than 0.4 of the radius from the surface, where the pressure is less than 1,000,000 atmospheres.

"To imagine a mechanical substitute for the Earth's constitution, without the introduction of pressure, suppose an alloy of adamant to give the material at the center of such a globe, of the same size but devoid of gravitation, a hardness three times that of armor plate. The outer layers as we approach the surface must then be supposed softer and softer, until it is like armor plate at a little over 0.6 from the center, and finally a very stiff fluid near the surface. In addition to this arrangement of its effective internal rigidity the actual Earth is enclosed in a spheroidal shell of solid rock analogous to granite. One can easily see that tidal forces applied to all the particles of such an artificial armored sphere would produce but very slight deformation, because of the enormous effective rigidity of the nucleus.

"The principal uncertainty in this result arises from the admissible variations in the assumed Laplacean distribution of density within the Earth. Both Radau and Darwin (cf. *Monthly Notices*, Roy. Astro. Soc., December, 1899) have pointed out that considerable variations in the internal distribution of density are possible without invalidating the well-known argument drawn from the phenomenon of the precession of the equinoxes; yet on physical grounds it seems clear that pressure is the principal cause of the increase of density towards the Earth's center. And since this does not vary greatly for moderate changes in the law of density, the principal of continuity shows that the actual law of density within the Earth cannot depart very widely from that of Laplace. The above value of theoretical rigidity of the Earth may therefore be taken as essentially accurate, and I think no doubt can remain that the rigidity of our Earth as a whole considerably exceeds that of steel. The original conclusions of Kelvin and Darwin are therefore confirmed by the present dynamical considerations based upon the theory of universal gravitation."

In this connection we should remember that the experimental

rigidity of steel is 808,000 and of glass 232,000 atmospheres. The calculated rigidity of all the matter within the globe, found by considering not only all the layers, but also the density in each layer, is found to be 1,028,702 atmospheres. Now the average rigidity must be greater than 750,000, because the stiffness of the crust and increase of viscosity downward is neglected in the gravitational method. In fact this method is not applicable to the outermost layers, because the pressure there is much less than the rigidity, and only becomes equal to the rigidity at a depth of something like one tenth of the radius, where the pressure is 320,295 atmospheres.

According to the experiments of Milne and Gray the rigidity of granite is about one sixth that of steel; and as steel has a rigidity of 808,000 atmospheres, that of granite is about 135,000 atmospheres, or a little more than one half of glass. We may therefore take the outer layers of our globe to have a rigidity about half that of glass, and assume that at a depth of 0.1 of the radius it becomes nearly 2.5 times as great as it is at the surface.

Whether it becomes at a depth of twenty miles less than it is at the surface we cannot tell, but such a decrease is not impossible, perhaps not improbable; because at this depth the molten rock moves in earthqakes, and yet in confinement it must have a very sensible rigidity, though probably not more than half that of granite.

Accordingly, it looks as if the rigidity at the surface is about half that of glass, at a depth of 20 miles about one half that at the surface, and at the depth of 40 miles nearly the same, but increasing below that depth and at 160 miles again equal to that at the surface, and at a depth of 400 miles considerably larger yet, or about 1.4 times that of glass. Increasing below this depth according to the pressure, it becomes at the center over 3 times that of nickel steel used in armor plate. The rigidity of steel is attained at a little over 0.3 of the depth to the center of the Earth. If this be the distribution of rigidity in the Earth, the curve of rigidity is as follows:

This postulated fall in the rigidity just beneath the crust is probable for several reasons:

1. The temperature increases quite rapidly as we go downward, while the pressure increases proportionately more slowly, so that a depth would be reached at which the matter would become a plastic if not a viscous fluid.

2. The eruption of volcanoes and lava flows on a vaster scale

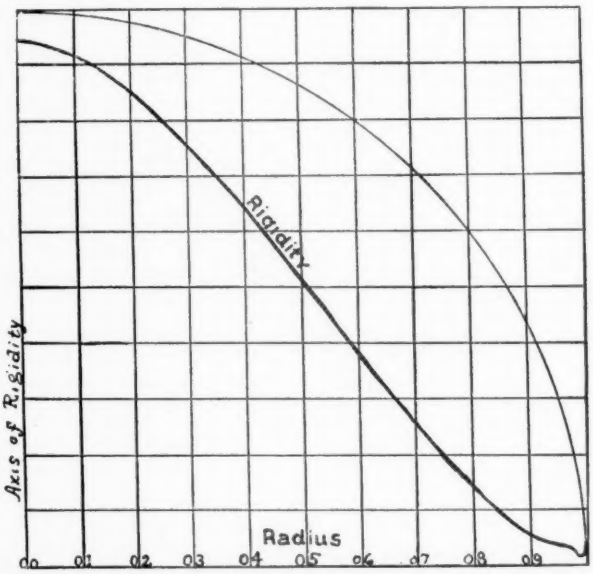
show that a molten layer underlies the crust, and occasionally is forced to the surface.

3. This underlying molten rock moves in world-shaking earthquakes, and frequently is expelled from beneath the sea under the land to form mountain ranges along the coast.

4. We may prove this expulsion of lava by the observed seismic sea waves which indicate a sinking of the sea bottom, and by the simultaneous uplift of mountains and coasts.

From these considerations it follows that the Earth is most nearly liquid just beneath the crust, and has the greatest rigidity at the center. As the plastic or quasi-viscous layer beneath the crust is thin, and possessed of considerable rigidity, *it too remains quiescent except when set in motion by the dreadful paroxysms of an earthquake.*

In tidal and other observations the Earth therefore behaves as a solid, and the rigidity of the Earth inferred by Kelvin and Darwin is confirmed. Yet a layer of plastic matter or quasi-viscous fluid exists just beneath the crust, and when disturbed by earthquakes gives rise to the development of ridges in the crust called mountains, chiefly by the expulsion of lava from under the sea.



CURVE OF RIGIDITY OF THE EARTH, SHOWING THE PLASTIC LAYER JUST BENEATH THE CRUST.

COLOR-PERCEPTION.*

F. J. B. CORDEIRO.

The Young-Helmholtz theory of color-perception, as well as that of Hering, and other theories, have all explained in more or less satisfactory manner the physiological color-phenomena. But on one certain point they all fail; some completely, others have had to be propped up by one new theory after another. It would be impossible in a paper of such limited length to give a full discussion of these different theories whose principal points may very likely be known to the reader. Since these theories were first formulated we have learned much about the mechanics of light and color. We have learned that there is an almost perfect analogy between all wave-phenomena, e. g. between sound and light: further, that the ether is a material substance with the property of transmitting electro-magnetic waves of any desired duration of vibration, and that light-waves are also only electro-magnetic waves of a certain length. [Hertz]. We have further learned that the resonance-phenomena in connection with all occurrences of vibration, be it with light or sound, are identical, and that these resonance-phenomena, that is, the property of taking up vibrations, depend only on the linear dimensions of the body and on the wave-lengths. With regard to this comparatively new knowledge we purpose to give an explanation of light and color-perception on a purely mechanical basis.

When Young in his theory declared that there were in the eye three and only three bodies sensitive to color, namely red, green and violet, he had no knowledge of electro magnetic resonance in the modern sense. Without doubt there are in the eye bodies which are sensitive to red, green and violet vibrations, that is, which, according to our new conceptions, are set in resonance by these vibrations. Also, according to the laws of resonance, it would perhaps be probable that all colors could be perceived with only three such bodies, yet not with such perfection as in reality takes place. With the laws of resonance we think that a body is capable of receiving vibrations which correspond to its dimensions not exactly but only approximately. But the resonance will be weaker in proportion as the divergence from the right dimensions increases, while it reaches a maximum when there is exact accord.

* Translated from the German by Miss Isabella Watson, Northfield, Minn.

The supposition, that only three bodies sensitive to color are found in the eye, corresponds fairly well to the observations; the supposition of four or six bodies would perhaps be better. But it would be best to examine the anatomy of the eye and see if it were not possible to discover the resonant bodies. If these bodies do not vary too much from the corresponding wave-lengths, we may hope that they will not be so small as to escape our examination, for under the microscope it is very easy to measure a wave-length. Through H. Miller's well-known experiment the shadows of the bloodvessels in the retina can be observed. Now since these vessels extend just to the layer of rods and cones but not into it, and since there is no nerve substance behind this layer, it follows that in this layer we must seek the end-elements, *i. e.* the light-receiving and color-refracting mechanism. If their dimensions are approximately or exactly like those of their corresponding wave-lengths, then we should be able to find them: and since the range of visible light amounts to something less than an octave, the smallest body should be, accordingly, about half as large as the largest.

Now we find that the cones consist of two parts—an inner segment, that stands on the "membrana limitans externa." and an outer segment that again is set upon the first. In the upper part of the inner segment is a clear strongly refracting substance which is separated from the other part by an ellipsoidal surface—the so-called ellipsoidal body. The outer segment of the cone consists of a row of disks which gradually grow smaller from the base to the point. A thin axial filament seems to run through the whole length [Ritter's filament]; yet this is not sure. The rods are constructed exactly analogous to the cones; they possess all their parts, but their forms are different. As the name indicates the outer segments of the rods are cylinders and not cones. The cones and rods form, crowded together side by side in a very fine mosaic, the cone and rod-layer of the retina.

Where the keenness of vision of the retina is the greatest, there are found only cones; just as in general the keenness of vision varies in proportion to the number of cones. Thus in the parts near the circumference—the "ora serrata"—, where there are almost no cones, there is also almost no sight, although the region is crowded full of little rods. We know already that every cone is capable of transmitting perception of light and color; hence we can assume, that only the cones are capable of it.

Zenker (1867), one of the first explorers of the retina with the perfected microscope, who well knew the wave-lengths of the

different colors, propounded the theory that in close analogy with the sound-waves, the graduated disks had a connection with color-perception. But Zenker did not follow up his theory with experiments or measurements; it was only a hypothesis. Now we will try to show that there are measurements which could support his view.

This hypothesis called forth but little notice and was forgotten until Stanley Hall restored it in reverse form.

Hall says: "Let us assume also for the present that these disks are arranged in a spectral order—those sensitive to red near the apex, those sensitive to violet near the base of the cone—each disk being transparent to all waves of greater length than those to which it is best fitted to respond." It is now necessary to consider the size and dimension of the cones in the background of the eye. In the *fovea* they stand very close together and form a very fine mosaic; here they are long slender bodies of 60—100μ in length, of which the outer segment takes up about the half. The inner segments have a breadth of 2—2,5μ; the outer a breadth of about 1 μ at the base which gradually grows smaller up to the point. When we leave the macula (or yellow spot on retina) the cones, no longer, lie beside each other, but they withdraw more and more from each other while the space between is filled out with little rods. Yet they remain regularly distributed. In these peripherical regions they are only 30—36 μ long and the inner segment amounts to two-thirds of the whole cone. Here the inner segments have an average breadth of 6—7 μ, but the breadth of the outer segment remains unchanged. The thickness of a disk at the base is 0,6 μ according to Max Schultze.

We will now weigh the proofs whether the cones are really the light-perceiving end-elements. If this is the case, then it follows that the smallest perceptible angle must correspond to the space between two cones lying side by side. Two points of light, e. g. two stars, can only be perceived as two different objects when they are 60″, or (for the sharpest eyes) 50″ distant from each other. According to that, their impressions on the retina, as Helmholtz has already shown, lie about 4,64—3,65μ from each other. We have now to see whether these distances correspond to those of the cones in the *fovea*. As has been already mentioned these distances are 2—2,5 μ, and at first thought one might consider this as the "minimum visible." If the diameter of an image were just exactly as large as the distance between two cones, then it might be possible that the

edge of the image should lie on both cones at the same time but it would be most improbable. If the diameter should reach just double the distance between two cones, then the image could touch one single cone only at one single point; and that could happen only for the shortest time. For cases lying between the two it is clear that the object is perceived sometimes as a point, sometimes as a surface. If one looks at two little wires at such a critical distance, one will always be in doubt whether one is seeing one or two wires; one moment we are quite sure of seeing two objects, the next moment we believe we can see but one, because the image lies first on one cone and then on two. The nearer we come to the upper limit the longer become the periods of certainty and the shorter the periods of doubt: then, thanks to the persistency of the perceptions, we will finally be observing two objects quite distinctly without the image having always to cover two cones. Now the least distance of two distinguishable retinal-images is 3,65—4,64 μ (Helmholtz) and falls somewhat below the doubled distance of two cones, which amounts to 4—6,25 μ. Here we have a direct proof that the cones are the actual end-elements.

Now if the outer segments are really the light-perceiving bodies, then the space between them must be dark. So that a very small point-formed image of the retina, as e. g. that of a star, will be seen only when it falls upon a cone, and it will be dark when it falls between two cones. If so tiny an image move back and forth over the mosaic, then it should be perceived as first light and then dark, or, as we are accustomed to say, it would twinkle. If we are observing a star of low elevation whose rays must suffer an irregular refraction in low layers of air, then its image wavers back and forth a little over the cone-mosaic and it does, in fact, twinkle. This gives a further demonstration of our statement as well as an explanation of the twinkling of stars.

Now the question arises, whether all rays of all colors must unite in exactly the same depth of the background of the eye, *i. e.* in the same plane, or whether—for the clearest focus—the image must lie in different planes according to the different colors. It is worthy of note that Helmholtz has always referred to the first supposition. He always writes about the plane of the retina, as though there really were such a plane, where all impressions are perceived most distinctly. And yet the layer of cones is 60—100 μ thick. As we shall at once see, the image of a colored object must lie in a very definite position or plane in

order to be seen most distinctly. If there be such a common mathematical plane for all impressions, then it is certainly not easy to conjecture where it lies. Fortunately, we have at our command the measurements in this respect of two of the greatest observers the world has ever seen: those of Frauenhofer and Helmholtz. Frauenhofer, the first to make experiments in this realm found a few moments irreconcilable with the supposition of a fixed plane of delineation; these he could not explain.

For the dioptric apparatus of the eye we can use the so-called reduced eye of Listing. In an eye of this kind, as is well-known, a single refracting surface is taken with a radius of curvation of 5,1248 mm, which separates water on the concave side from air on the convex. For purposes of reckoning such an eye replaces the human eye with sufficient accuracy. Now Frauenhofer experimented with red light which answers to his line C, and violet answering to his line G. He had already determined that these rays had for water an index of refraction of 1,331705 and 1,341285 respectively. For parallel red rays the focusing point lies on the axis of our reduced eye 20,574 mm behind the vertex of the refracting surface; for parallel violet rays the focus lies 20,140 mm behind this area. Hence the focal planes of these two colors are 0,434 mm distant from each other. (Helmholtz Ph. O. §13). If the violet focal plane is to coincide with the red, the violet rays must needs come from a distance of 713 mm in front of the refracting surface. For this reason, Frauenhofer supposed that one must see the violet illumined object most distinctly at this distance; but in fact he had to bring it to a distance of 685 mm. He took note of the variation but could not explain it. Helmholtz too carried out similar experiments and likewise ascertained this deviation. He could not explain it either, but he supposed that it depended upon a difference in the refracting power of the eye and of water, although according to all measurements, they are not to be distinguished. Matthiessen also reached these results (Ph. O. a. a. O.)

We will presuppose that there is only a slight difference or none at all between the refracting power of the eye and of water. In this case the violet image in Frauenhofer's eye must have lain 37 μ behind the red. Frauenhofer's experiments were extremely exact. The data of his lines C and G make possible the exact determination of the distance between the two images.

Helmholtz does not give with exactness the parts of the spectrum which he compared; he merely says "violet and red." Let

us assume that he used Frauenhofer's line C for red and the extreme violet with a refraction index of 1,3425; then the distance between the two images was 30 μ. The measurements of Matthiessen give about 40 μ. So the observations of all three agree very well, and on the supposition that the power of refraction of the eye is very nearly the same as that of water, we notice that the violet focal plane lies behind the red at a distance of the length of the outer segments of the cones. In a series of experiments with colors of the spectrum I was able to demonstrate the above results very well, and also that the other colors (yellow and green) consequently lie between these limits.

On the ground of these proofs then we will assume that a red image lies in a plane which intersects the large disks, a violet one in a plane which intersects the smallest disks; the other colors form planes intersected by the resonant disks corresponding to them. Here we have to deal with a physiological constant magnitude in the human eye that corresponds to an anatomically physiological magnitude, viz., the length of the outer segments.

Von Gräfe declared, that in aphacia there was a certain range of accomodation though very short. Donder, on the other hand, says: "The accomodation line of Czermaks which is mistakenly connected with the length of the rods depends upon this: it is caused by the lack of symmetry of the dioptric apparatus of the eye and is a function of the length of the focal power." But in spite of this I believe that Von Gräfe is right, and that the short range of accomodation, if one may call it so, really is caused by the different levels of the disks.

In the following we will consider the disks as elastic bodies, each of which has its own period of vibration, and which are capable of taking up this period or a great number of them. From their size we expect that these periods will reach over an octave, and in fact we find that visible light includes from 395 to 760 billion vibrations. In physics it sometimes happens that a coincidence arises not from chance but from a common law. In all probability pure *chances* happen very seldom; *e. g.* Maxwell found that the velocity of a disturbance in an electromagnetic medium agreed exactly with that of light. Later experiments in that line have proved that it was no chance, but identity of the phenomena.

To be continued.

A CRITICISM OF "FAITH AND THE FOURTH DIMENSON."

WILLIAM H. JOHNSTON.

For Popular Astronomy.

A reading of "Faith and the Fourth Dimension" suggests to me that, in making public his ideas on that subject, Professor Jacoby has failed to appreciate the obligations imposed by his position. As a representative of the intellectual life, he owes it to his station to confine his utterances to a subject in which his reason can withstand his prejudice and in which his knowledge of the facts justifies an expression of opinion. That he has not so confined himself will be made evident by a consideration of his article.

The author's efforts are devoted to showing that "it is possible for science, like religion, to believe something not logically proven." Grant his contention that science takes things on faith; does it follow that science is, therefore, like religion? One man believes that the stars influence human character; another believes that space is three-dimensional in points. Neither can prove his case; but, surely, they are not equally credulous. In other words, believing one thing without proof is not necessarily on par with accepting some other thing without proof. Hence, religious faith is not necessarily the same as scientific faith; and since Professor Jacoby has not shown that they are alike, he is not justified in saying that one resembles the other.

Let us, now, look to the accuracy of some of the facts. According to our author, Lobachevski has shown "that Euclid's demonstrations are not in accord with the extreme requirements of rigid logic." As a matter of fact, the rigor of Euclid's logic is questioned no more to-day than it was two thousand years ago; for in mathematics "no new development can undo the work of previous developments or substitute new in place of old results." (Herman Schubert: On the Nature of Mathematical Knowledge.)

In particular, we are expected to believe that Lobachevski's theorem regarding the sum of the angles of a triangle conflicts with Euclid's angle theorem. If we draw a triangle on a sphere, the sum of the angles is not 180°. But who imagines that the theorem is at variance with the one about the plane triangle? And if no one is surprised to learn that a figure on a curved surface does not possess the same properties as a

figure on a plane, one ought not to be especially astonished to know that Lobachevski's results differ from Euclid's; for the former's triangle is on a curved surface. "One of the most surprising results of modern geometric investigations was the proof of the applicability of the non-Euclidean geometry to pseudospheres or surfaces of constant negative curvature." (Dr. Karl Fink: A Brief History of Mathematics.)

The next topic which is of interest to us in this connection is the fourth dimension, the domain of "half ridiculous phenomena." An elaborate discussion is hardly necessary here, for a mere suggestion of one phase of the subject as it appears to a mathematician will suffice to show that the fourth dimension is not so much a figment of the mathematical imagination as Professor Jacoby would have us believe. To begin, it is necessary that we realize definitely what we mean when we say that space is three-dimensional in points. An answer involving the terms length, breadth, and thickness is not satisfactory; for, what is the length of space? If we think of a portion of space, what is the thickness of a sphere? Such questions do not occur if we say that space is three-dimensional in points because to locate a point one must know three things about it. These three may be latitude, longitude, and altitude.

Suppose that, instead of limiting ourselves to points and lines, we turn to the geometry of spheres and groups of spheres. From the point of view of such geometry, what is the dimensionality of space? That is, how many things must one know to locate a sphere, which is the element of space? A particular sphere may be fixed by determining its center and radius. Three qnantities fix the former; and hence, four distinguish one sphere from all others. Thus, when we study the geometric properties of groups of spheres, the space of our senses is four dimensional. If, then, the space that we know may be either three or four dimensional, the fourth dimension cannot be any further from reality than the third.

Up to this point we have considered the illogical structure of "Faith and the Fourth Dimension," the erroneous idea of the author regarding Euclid and Lobachevski, and, finally, the writer's limited knowledge of the fourth dimension. There are one or two other matters which might be of interest, but I think enough has been written to justify the statement that Professor Jacoby has not sustained the intellectual standard of his position. I have sought to establish this proposition, not because the article is exceptional, but because it seems likely that

if such men as Sir Oliver Lodge could be led to appreciate the true worth of their utterances in defence of opinions born of prejudice they might be less willing to exhibit their intellectual infirmities; and then the prestige of learning would, no longer, aid in the propagation of ignorance.

Brooklyn, N. Y.

THE EIGHT-CENTERED OVAL AND ELLIPSE.

FREDERIC R. HONEY.

By a careful selection of radii, an eight-centered oval may be drawn which will very nearly coincide with the ellipse constructed on the same axes, and may, within wide limits, be accepted as a representation of it. By this method the use of the curve ruler is avoided, and symmetry with respect to the axes is maintained. The construction given below is the result of an extended series of observations upon eight-centered ovals constructed on axes of various proportions, and a comparison of these curves with the corresponding ellipses.

In drawing an eight-centered oval, three radii are employed. With the shortest radius we describe the two arcs which pass through the vertices of the major axis; with the longest, the two arcs which pass through the vertices of the minor axis; and with the third radius the four arcs which connect the former; *i. e.*, the figure is an assemblage of eight arcs of circles.

Fig. 1 represents an ellipse with the osculating circles—or circles of curvature—at the vertices of the minor and major axes.

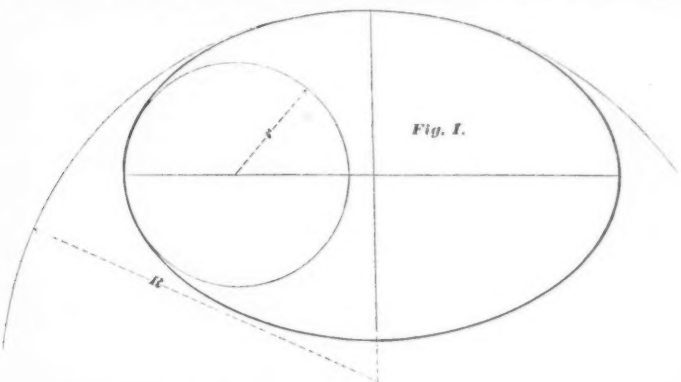


Fig. I.

A simple method of determining the radii of curvature is illustrated in Fig. 2. Draw the straight lines *a f* and *a c* forming *any* angle at *a*. With *a* as a center, and with radii *a b* and *a d* respectively equal to the semi-minor and semi-major axes, draw the arcs *b e* and *d c*. Join *e d* and through *b* and *c* respectively draw *b g* and *c f* parallel to *e d* intersecting *a c* at *g*, and *a f* at *f*; *a f* is the radius of curvature at the vertex of the minor axis; and *a g* the radius of curvature at the vertex of the major axis.

From the similarity of the triangles *a c f*, *a e d* and *a g b*, the student will see that this construction is in conformity with a demonstration in the calculus, viz., that the radius of curvature at the vertex of an axis is a third proportional to the semi-axes. With these radii (*R* and *r*) the osculating circles in Fig. 1 are described.

One of these circles falls wholly *without* the ellipse, while the other falls wholly *within* the curve. It is evident then, that in order to represent an ellipse approximately by arcs of circles, the longest radius should be less than *R*, and the shortest radius greater than *r*.

The following empirical construction gives the best result. Lay off *d h* (Fig. 2) equal to one eighth of *b d*. Join *e h*, and draw *c k* and *b l* parallel to *e h*. Take *a k* for the longest radius

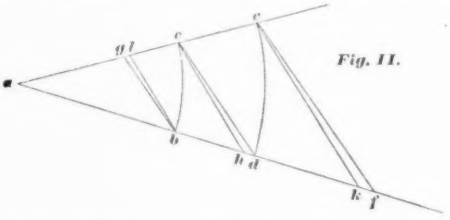
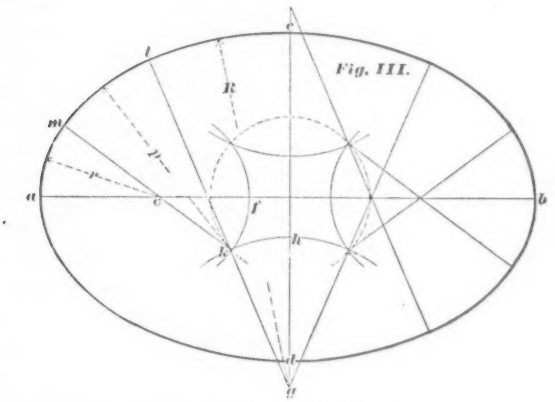


Fig. 11.

(=*R*); *a l* for the shortest radius (=*r*); and the arithmetical mean, or one half the sum of the semi-axes, for the third radius (=*p*); and employ these radii in the well-known construction for the eight-centered oval.

In case the student may not be familiar with this figure it is illustrated. Let *a b* and *c d* (Fig. 3) be the major and minor axes. Lay off *a e* equal to *r*, and *a f* equal to *p*; also lay off *c g* equal to *R*, and *c h* equal to *p*. With *g* as a center and *g h* as a radius, draw the arc *h k*; with the center *e* and radius *e f* draw the arc *f k* intersecting the former at *k*. Draw the line *g k* and produce it, making *g l* equal to *R*. Draw *k* e and pro-



duce it, making $k\ m$ equal to p. With the center g and radius $g\ c\ (=R)$ draw the arc $c\ l$; with the center k and radius $k\ l\ (=p)$ draw the arc $l\ m$; and with the center e and radius $e\ m\ (=r)$ draw the arc $m\ a$.

Since the remainder of the work is symmetrical with respect to the axes, the student will need no explanation beyond that which is afforded by the drawing.

Hartford, Conn.

AN ASTRONOMICAL THEORY OF THE MOLECULE AND AN ELECTRONIC THEORY OF MATTER.

Solar and Terrestrial Physics Viewed in the Light Thereof.

SEVERINUS J. CORRIGAN.

FOR POPULAR ASTRONOMY.

Part III. (Continued.)

THE NATURE OF THE SOLAR RADIATIONS AND THEIR RELATION TO TERRESTRIAL MAGNETISM AND OTHER METEOROLOGICAL PHENOMENA.

Upon the orbital velocity, represented by $D^{\frac{1}{6}}$, or $P^{\frac{1}{4}}$, depend the rigidity and elasticity of the molecules of all gaseous matter, these quantities being proportional to the square of the velocity and therefore to $D^{\frac{1}{3}}$ and $P^{\frac{1}{2}}$ and, even in the case of that superlatively tenuous matter called the "luminiferous

ether," which is at the lowest known, (and the fundamental) temperature in nature, the molecular rigidity and elasticity are very great because, as has been demonstrated, the atoms of each molecule of this transmitting-medium for radiance of all kinds are continually revolving, in their normally circular orbits, with a *linear* orbital velocity equal to 2π times the "velocity of light" which is (according to Professor Newcomb), very approximately, 186,340 miles, per second which expressed in English feet, per second is 9839×10^5 (in *centimeters* 2999×10^7, or 3×10^{10}, very nearly) the orbital velocity being therefore, 6182×10^6 feet, or 1884×10^8 centimeters, per second, these quantities being 2π times the "velocity of light" which is the projection of this orbital, or tangential, velocity of the atoms, upon the diameter of the circular orbits thereof, or upon the diameter of a molecule of the ether,—the velocity of light being, obviously, a radial quantity. The molecules of the atmospheric gases at the Earth's surface are by reason of the compression caused by the terrestrial force of gravity, very much denser and of smaller radius, and possess, therefore, a much higher absolute temperature and pressure (due to the much greater orbital velocity of their component atoms) than the molecules of the fundamental matter, the luminiferous ether the density whereof, relative to that of air under the normal conditions, is extremely low, and the linear orbital velocity of the atoms of the normal atmospheric molecule is proportional to $D_a^{\frac{1}{6}}$; ie., to the sixth root of the density (D_a) of atmospheric air, relative to the density (D_e) of the ether, which, in this connection, is taken as *unity*, D_a being therefore the *reciprocal* of the density D_e. The last named density is, according to my determination, set forth in a preceding part, $5191 \times^{-20}$, or $\frac{1}{1412 \times 10^{13}}$, whence the value of D_a is 1412×10^{13} the logarithm of which is [16.149448] the logarithm of $D_a^{\frac{1}{6}}$ being therefore, this logarithm divided by 6, or [2.691658], the natural number whereof is 491.65, the linear orbital velocity of the atoms of a molecule of atmospheric air being 3040×10^9 feet, or 9266×10^{10} centimeters, per second.

Although the orbital velocity of the atoms of the mean atmospheric molecule, under the normal conditions, is thus nearly 492 times that of the atoms of the molecules of the luminiferous ether, the velocity of transmission of radiance by these

atoms of the comparatively much denser matter of the atmospheric gases, is sensibly the same as when the transmission is through the medium of the atoms of the ether out in space, at an altitude of very approximately 250 miles above the Earth's surface, at which height the matter of the terrestrial atmospheric envelope is in equilibrium, as to density, temperature and pressure, with the tenuous matter of the luminiferous ether, this altitude marking the upper limit of the Earth's atmosphere as determined by an analytical method based upon the principles of my theory of gases, and sufficiently well verified by observational data of several kinds, as will be shown subsequently.

A fundamental analytical expression derived from the principles of my theory is $V_1 = \frac{\lambda}{A}$ in which λ represents wave-length, and A the corresponding amplitude of vibration, in the case of luminous radiation through the transmitting medium, the "velocity of light" being designated by V_1 which is, as is well-known, sensibly a constant quantity. By reason of the greater orbital velocity of the atoms of the atmospheric gases, the "frequency of vibration" is increased in direct proportion and the wave-length λ decreased in the same ratio, when radiation is transferred to the denser molecules of the atmospheric gases under the normal conditions of temperature and density, as is indicated by the well-known expression of the "wave-theory of light"; that is $V_1 = f\lambda = a$ constant, in which f represents the frequency of vibration.

Therefore, the wave-length, (λ) in the case of luminous vibrations through the normal atmospheric molecules, is only $\frac{1}{491.65}$ of that when the transmission takes place through the atoms of the molecules of the ether in outer space, the orbital velocity of these atoms being less than that of the ultimate particles of the standard atmospheric molecules, by that fraction; but, by reason of the greater orbital velocity of the latter atoms, whereby the rigidity of each molecule is increased, any given impulse transferred to an atom of the medium by impact from any ultimate particle of a radiating surface, will deflect this atom from the tangent to its normally circular orbit, at the point of impact, by only the $\frac{1}{491.65}$ part of the deflection that would result from the same—or an equal—impulse or impact directed against a revolving atom of a molecule of the ether, the circular function defining the angle of deflection being

the cotangent thereof, which also represents the amplitude of vibration designated by A, which, therefore, in the case of the atmospheric gases aforesaid, is $\frac{1}{491.65}$ of its value when the radiation is through the ether, and since it varies *pari passu* with λ, there results the equation; $V_1 = \frac{\lambda}{A}$ = a constant, which is an expression of the undulatory theory as well as of mine.

This is only one of several links connecting the wave theory of radiation, with the one that I have advanced, mine being, in this respect, a veritable undulatory, as well as corpuscular theory, the Newtonian theory (which was long ago discarded for that of Huygens, when the "criterion" as to the relation between the velocity of light in media of different densities, and the index of refraction was applied) which is corpuscular, being thus corroborated in so far as the nature of the medium, by, and through, which the radiation is propagated, is concerned, the luminous vibrations being those of almost infinitesimal material particles, the atoms—while the motion of these particles is in accordance with the principles of the wave theory advanced by Huygens, the great Dutch scientist of the 17th century.

The values of the linear, orbital velocities of the atoms of the molecules of the atmospheric gases (at the standard absolute temperature, 491.65 degrees, Fahrenheit (32°), and pressure 14.73 pounds (avoirdupois per square inch), which velocities are 3040×10^9 feet, and 9266×10^{10} centimeters per second, should be well noted because they serve as the basis of the greater part of the numerical determinations from the principles of my general theory in regard to the constitution of gases and the nature of radiation through the medium of gaseous matter, and particularly in regard to the constitution and extent of the terrestrial atmosphere, its relation to the luminiferous ether, and the functions that its ultimate particles, or atoms, perform in the processes of radiation and of electric and magnetic action, the "dielectric strength of air", being obtained directly, and simply—as has been demonstrated—from the atmospheric pressure (grams per square centimeter) from which quantity I have derived the several electrostatic, electromagnetic and electrochemical units, in the case of terrestrial magnetism, in absolute measurement according to the C G S system, the results of my investigation, in this connection, leading directly to a knowledge of the very nature of the electric and magnetic forces which have,

heretofore, been known only by their effects. It is these results,—the derivation whereof has been set forth and discussed in the preceding number—that have led me to designate my theory as "an electronic theory of matter," in addition to that title which indicates its astronomical aspect.

Reverting to the discussion of electro-magnetic action and the equations pertaining thereto—which are of prime importance in my theory of the nature of the solar radiations—in the October number of POPULAR ASTRONOMY, attention is called to the following facts. The value of the absolute unit of electromotive force, given by equation (*ι*) on page 502 of that number, is expressed in gram-centimeters, per second, and it represents a tangential force, but it is reducible to a radial quantity through division by 2π, while a further division by the terrestrial force of gravity (g), expressed in centimeters per second the number whereof is 980.7, will reduce it to dynes, per square centimeter, the resulting expression being for the greatest possible number of "lines of force" in that area for unit inducing current, or the maximum magnetic "induction" as it is called. The resulting equation for the maximum induction (I') as derived from the aforesaid equation for unit electromotive force, is $I' = \frac{Pa\, V_1^2}{61620\, d^2}$, in which Pa represents the normal atmospheric pressure in grams per square centimeter, V_1 the velocity of light, and d, the *reciprocal* of the normal molecular diameter, in centimeters, and it should be noticed that this is the significance of the literal factor d in the equations on page 502, while in some other cases, as on page 506 of the November number, it represents the molecular diameter directly.

A solution of the equation for I', aforesaid, with the numerical values of the several literal factors in the right-hand member, taken from the table on page 501 of the November number, gives, roundly, 18,000 as the maximum number of "lines of force" per square centimeter this being only a little greater than the maximum in the case of soft iron which possesses the greatest magnetic "susceptibility" of any known substance, the maximum magnetic induction in this case being 17,500 lines of force per square centimeter, according to the experimental determinations of Rowland. As the inducing current is increased from a low value, the magnetic induction increases, but not pari-passu, or indefinitely, a maximum value being soon reached, when the magnetized metal is said to be "saturated", or in other words, when it cannot have its magnetism further in-

creased by augmentation of the inducing current, all of the atoms of all the molecules in the "interface" being then affected according to my theory in this connection which I think, explains this peculiar and well-known condition of magnetism, in a rational and satisfactory manner.

Under my theory the specific nature of the magnetic and electrostatic forces, particularly in regard to what is called "polarity" which is their distinguishing characteristic, becomes definitely explicable as follows:

As I have pointed out, each normal, undisturbed molecule is composed of a definite number of entirely similar atoms, the number (n) thereof being expressed by $2.\frac{d}{\Delta}$ the diameter of the molecule being designated by d and that of an atom by Δ and all these atoms are equally distributed in each molecule under the normal conditions, and are moving with equal velocity around their respective molecular centers, in all directions, there being as many atoms in one hemisphere of each spherical molecule as in the other, but when by the application any force, mechanical, such as that of friction, percussion or compression; of chemical affinity as in the galvanic cell, or of heat to a sufficiently high degree, this equilibrium of any molecule is disturbed, or destroyed, by the addition of atoms above the normal number, or by subtraction from said number, the molecule having an excess of atoms will be "*positive*" with respect to one in which the atoms are in defect, the latter being "*negative*", a condition of "polarity" being thereby established and there will be a tendency to a restoration of equilibrium by the transfer of atoms from a positive molecule, wherein they are in excess, to a negative one in which they are in defect, the former thus possessing a "potential energy" analogous to that of a coiled spring. There is thereby produced a tension such as that designated by "potential", so that when the recoil takes place in the process of restoration of mechanical equilibrum in the molecules, the latter will be wholly, or partially, broken up by the action, the orbital velocity of their atoms being increased from the normal circular velocity to, or above, a velocity that would send them off in an open curve, a *parabola* or an *hyperbola*.

Complete, and practically instantaneous, dissociation of the atoms of a molecule constitutes a "disruptive discharge"—such as that of static electricity and the lightning flash—while a succession of these discharges, at almost infinitesimal interv-

als, constitutes a continuous electric current. Since each normal molecule of the medium, under my theory, is practically a spherical shell enclosing the only absolute void in the known universe which,—even as Democritus the philosopher of old has stated,—consists, in the ultimate analysis, of "atoms and a void" the complete disruption of a molecule and collapse of the molecular shell, must be accompanied by detonations varying in loudness and intensity from the feeble crepitations caused by discharges of electricity generated by ordinary electrical apparatus, to the crash of thunder, consequent upon the lightning flash which is caused by the disruption of a long line of atmospheric molecules, the acoustic effects of a complete collapse of a molecule of the gaseous medium being quite analogous to the report that follows the sudden breaking of the bulb of an incandescent electric lamp from which the air has been exhausted as far as possible.

The statements in the immediately preceding paragraphs refer only to the cases in which all the atoms of a molecule are affected by extraneous impulses, the atoms of all the atomic couples dissociated and the molecule completely disrupted, but when a portion only of these atoms in a molecule are affected and the molecule itself is not destroyed, the resultant action is productive of what is called magnetism, and electro-magnetism, where the disturbing force is that of an inducing electric current.

Taking the case of a single layer of molecules, (the thickness whereof is the molecular diameter (d)) in most intimate connection with the surface of a bar of soft iron, and forming an "interface" between said surface and the molecules of the medium beyond, it is obvious that, when undisturbed by extraneous forces, all the molecules in this layer are in dynamic equilibrium, each being composed of the same normal number of like atoms distributed equally around the whole molecular surface, and revolving with the same orbital velocity around each molecular center, each molecule being therefore endowed with the same quantity of kinetic energy which is a function of the mass of the atoms and the square of their velocity. Now, let this equilibrum of the molecules be disturbed, either by the vibrations caused by an inductive electric current passing through conductors coiled around the bar, or by frictional, or other, means as in the case of static electricity, it is obvious that some of the molecules must be supercharged above the normal, with atoms expelled from a normal molecule, by reason of increase of orbital velocity, and since the normal number of molecules in the un-

disturbed layer aforesaid is constant, these ejected atoms must gather, or crowd together, in excess in the molecules at one end of the bar, thereby causing a deficiency at the other end, the former end constituting the positive magnetic pole, and the latter the negative pole. The supernumerary atoms will be forced outward from the positive pole, or end, of the bar, but as all surrounding molecules of the medium are in possession of the normal number of atoms, the only place that these wandering corpuscles or "ions"—as we may call them—can find lodgment is at the other end of the bar which constitutes the negative magnetic pole, where the molecules have less than the normal number of atoms, and are, consequently, deficient in kinetic energy.

The atoms thus expelled at first pass directly outward from the positive pole, or end of the bar, and then curve and move backward along the lines of least resistance, roughly parallel to the axis of the bar to the other end thereof, then re-curving, and moving inward to the negative pole, in the tendency to the restoration of molecular equilibrium in the interface, and there will thereby be established a "magnetic circuit" in a manner quite the same as in the case of the complete disruption of the molecules, that is the cause of electrical action. It is obvious that, under these conditions, if the positive poles of two such bars be brought into juxtaposition, the expelled atoms from each will be directly opposed in the direction of their motion, and a force of repulsion result, whereas if a positive pole be presented to a negative, the outflow of atoms from that end of the bar will coincide with the inflow from the negative pole of the other bar, the reaction obviously being such that the two bars will then experience a force of attraction; in other words like poles repel, and opposite poles attract, each other, which is only a statement of an axiom in Magnetism. The atoms thus moving along the line of least resistance from the positive to the negative end of the bar, constitute what are known as lines of force, and a similar action occurs in the case of induced static electricity, the phenomena whereof are explicable as in the case of magnetic induction aforesaid. It is thus apparent that, under my theory, electrical and magnetic action can be excited only by the application of some extraneous force to the molecules, and therefore that even though there were no wasteful loss, or dissipation, of energy, no more work can be derived from the kinetic energy of the atoms of these molecules—enormous as it is even in a cubic centimeter of gaseous matter—

than is expended upon them, this principle being analogous to, and axiomatic as, the well-known one of thermodynamics, in this respect, which is demonstrated by the working of a heat engine of any kind.

In the case of statical electricity as artificially generated directly by divers apparatuses, and also in that of the conductors on the rapidly revolving armature of a dynamo, the applied forces are those of friction, percussion, and compression, and in the galvanic cell it is that of chemical affinity and its reaction, while in the case of thermo-electricity as generated in the thermopile, and also in that of the solar electro-magnetic radiations, the generating force is that of "heat." As I have demonstrated in the latter case the effective surface, or radiating, temperature of the Sun, (6,700 degrees Fahrenheit absolute) is just above the absolute temperature corresponding to an orbital velocity augmented by the velocity of impact of the atoms of the radiating surface, to the parabolic limit at which the atomic couples of the molecules of the transmitting medium are dissociated, this limiting temperature being 6680 degrees Fahrenheit, and since the density of the transmitting medium in space, which for convenience may be called—as it is—the luminiferous ether is almost infinitesimal as compared with that of the normal atmospheric gases, the density of the latter being, as I demonstrated, 1412×10^{13} times as great as that of the ether, if we take the latter density as the unit in this connection, (reference being made, of course, to volume-density as expressed by $D = \frac{I}{V}$ and not to the density involving the actual mass (M) as expressed by $D = \frac{M}{V}$), as I have demonstrated, the rigidity of a molecule, in so far as it is due to the orbital velocity of the atoms revolving around the molecular center, is a function of the volume density (D) of the molecule, or of mass of gaseous matter composed of these spherical shells, it being proportional to $D^{\frac{1}{3}}$ which represents the square of the relative orbital velocity of the atoms in media of different volume-densities, the orbital velocity itself being proportional to $D^{\frac{1}{6}}$, the numerical value whereof is 491.65 (which is also the absolute temperature, in degrees Fahrenheit, of the standard atmospheric gases under the normal conditions, viz., 32 degrees Fahrenheit) and the square of this, or $D^{\frac{1}{3}}$, is roundly 241,700, the logarithm of

which is 5.383312—that of the relative density aforesaid being 16.149936.

A velocity of impact of an atom of a radiating surface, directed against an atom of a molecule of a surrounding gaseous medium (this impact and the resulting additional velocity being imparted primarily to an atom of a molecule in the layer or interface in immediate contact with the radiating surface) and corresponding to an absolute temperature of 6680 degrees Fahrenheit, is just sufficient to dissociate the component atoms of an atomic-couple in a molecule of the atmospheric gases even when at their normal rigidity at a pressure of one atmosphere, or 14,730 pounds per square inch at the Earth's surface, the complete disruption of these molecules and the dissociation of their component atoms by the energy due to the application of a velocity of impact which augments the normal orbital velocity to or beyond the parabolic limit corresponding to the aforesaid absolute temperature, being exhibited in a most striking manner in the case of the voltaic-arc between the carbon points, or poles, of an arc lamp when an electric current of sufficiently high voltage is coursing through the circuit, the temperature of the arc thus formed being very approximately, according to the ordinary Fahrenheit scale, 6220 degrees which is equivalent to the absolute temperature of dissociation of the atoms (6680 degrees) productive of electric action.

Since the orbital velocity of the atoms in each molecule of the luminiferous ether is so much less than in the case of the normal standard atmospheric molecules, it being only $\frac{1}{491.65}$ of the velocity in the latter case, it is obvious that—the molecular rigidity in the case of the ether being only the square of the fraction just stated, that the ethereal molecules will be disrupted by a velocity of impact from the atoms of a radiating surface, corresponding to an absolute temperature much less than that in the case of the limiting absolute temperature at which the atoms of the molecules of the ether are thrown off in parabolic curves, it being only, roundly 3,760 degrees Fahrenheit, or 2,920 degrees less than in the case of the atmosphere as I have determined from the principles of my general theory as to the constitution of gases, the nature of radiance and the law of radiation founded thereon, in the following manner.

In the process of radiation the velocity of impact (v) of an atom of the radiating surface, which impact is longitudinally

directed against an atom of the radiating surface which is necessarily a solid (although the radiating material may be very finely divided) the atoms whereof are vibrating longitudinally and impinging upon the circularly revolving atoms of the molecules of the medium in the layer or interface in immediate contact with said surface, the "velocity of impact" (v) of any atom of the surface increases with the absolute temperature of the radiating material, the velocity being added to the normal orbital velocity of the atoms of the medium and the impact deflecting any affected atom from the tangent to its circular orbit at the point of impact, by an angle that may be designated by (χ) which varies with the velocity (v) and thereof with the radiating temperature, degree by degree. The deflection thus caused by the impact, and the increased orbital velocity, throws the atom of the medium from the normally circular orbit into an elliptical one the eccentricity increasing with (v) up to the point at which this velocity of impact is just equal to the normal orbital velocity which is increased thereby to 1.414 times its normal value, when the eccentricity becomes 1 and the orbit parabolic, the ratio of increase aforesaid being $\sqrt{2}$, which expresses the well-known relation between the velocity of a planet at unit distance from the Sun, and the corresponding parabolic velocity at the same distance, as is demonstrated in Analytical Mechanics and Theoretical Astronomy. We know therefrom that if the orbital velocity of the Earth at its present mean distance from the Sun were increased in the aforesaid ratio from nearly nineteen miles per second to nearly twenty-seven miles in the same time, our globe would leave its elliptical, and nearly circular, orbit around the Sun and move off in a parabolic curve that would carry it forever away from the center of gravity of the Sun the attractive force of the latter body being unable to hold the Earth in its closed orbit if our globe were to have its orbital velocity increased to twenty-seven miles per second—which is also the velocity of any comet moving around the Sun, in a parabolic orbit, when at the mean distance of the Earth from the center of gravity of the solar globe. The deflection of the path of the affected atom of a molecule in the layer in contact with the radiating surface, from the tangent to the normally circular atomic orbit at the point of impact, is in the direction of the ray which lies along the diameter of the spherical molecule at that point, the deflected curve extending from this point to the other end of the molecular diameter, 180 degrees distant, where the affected atom delivers up the added

velocity of impact (v) to a revolving atom of a molecule farther on in the direction of the ray and contiguous to the affected molecule in the interface aforesaid, this process being extended outward from molecule to molecule by means of the revolving atoms constituting these spherical shells (this being the process known as radiation) the vector-atom, after delivering up its impressed velocity and resultant burden of kinetic energy to the atom in the molecule in advance, returning to its circular orbit and, through the other semi-circle, back to the original point of impact, each atomic orbit becoming again circular until another similar impulse is given from the atom of the radiating surface. When an atom, or a large number of them as is actually the case in a molecule, is so affected, the molecule is deformed, and reformed when the velocity has been delivered up, as aforesaid, to the atoms of a contiguous molecule farther on along the ray. But this process—which is productive of what we know as "heat" and "light"—can continue only up to the parabolic limit, corresponding to a radiating temperature of 6680 degrees Fahrenheit, as aforesaid, in air at normal pressure density, and temperature, and of 3760 degrees Fahrenheit in a medium at the density and temperature of the luminiferous ether, as determined through my theory, and at this point the angle of deflection (χ) of the path of the affected atom from the tangent to its normally circular orbit becomes 45 degrees (the natural tangent and cotangent whereof are each 1) and the affected atoms are thrown off in open curves, the path of each such atom becoming hyperbolic beyond that limit as the respective radiating temperatures are increased, the hyperbolic curve gradually approximating, and finally becoming, a straight line coincident with the diameter of the molecule, the orbital revolution of these atoms becoming then longitudinal vibrations similar to those of the X-rays and others of that type electrically generated, instead of being transverse as are thermal and luminous vibrations which are produced when the atom is thrown from its normally circular orbit into an elliptical one only.

In the case where atoms are expelled from each molecule of the medium affected by the radiation, these expelled atoms eventually recombine in a molecule that possesses less than its normal number—as expressed by $2.\frac{d}{\Delta}$—the dynamic equilibrium of a molecule being destroyed when its atomic components fall below, or rise above, the normal number and restored when recom-

bination takes place in the process just described. As has been pointed out on a preceding page, if only a portion of the component atoms, diatomically arranged in a molecule, are dissociated, the spherical molecular shell yet retains its form, but the molecular kinetic energy thereof is decreased by the reduction of the number of atoms below the normal quantity, the resultant effect being in this case magnetic, or exemplified by the action in the case of a bar of soft iron around which an electrical current is conducted, and by what we know as terrestrial magnetism which results from the partial disruption of the layer of atmospheric molecules contiguous to the Earth's surface, the prime cause of this disruption being the velocity of impact imparted to the atoms of these molecules by the radiation from the Sun, through the molecules of the intervening medium which is called the luminiferous ether, this velocity corresponding to a radiating temperature above that at which the atoms of the molecules are dissociated (6680 degrees Fahrenheit) the effective surface temperature of the Sun being 6700 degrees Fahrenheit according to my determination; but when all the atoms of the molecule are dissociated and all the molecules in unit surface of the molecular layer aforesaid are disrupted, the resultant action is electrical as illustrated by disruptive electrical discharges such as those produced artificially by divers apparatuses and means, for example, the discharges from a frictional electrical machine, a Leyden jar, and in vacuum tubes, such as those of Crookes and Geissler, and in nature by the lightning flash and the aurora borealis or northern lights, as it is commonly known, this last named phenomenon being analogous, in all practical respects, to the phenomena exhibited in the case of discharges—either direct or indirect—in the vacuum tubes aforesaid and these facts enable us to easily comprehend and analyze the specific nature of the solar radiations.

The force of gravity at the Sun's surface is somewhat more than twenty-seven times that of terrestrial surface gravity, so that were the normal temperature the same as that of the atomspheric gases at the Earth's surface, which is taken at 32 degrees Fahrenheit or 492 degrees as measured on the absolute scale, the density of the gaseous envelope immediately surrounding the Sun just above the photosphere and in contact therewith would, by the compression due to the solar surface gravity, be a little more than twenty-seven times that in the similar case of the terrestrial atmospheric gases under the normal conditions which I have taken as the standard, of density (D), pressure (P), and

temperature (T), the relations between these quantities being expressed $P = DT$ whence $D = \frac{P}{T}$. But, according to my determination the maximum absolute temperature of the gaseous matter composing the solar globe, which temperature has been generated by compression of the solar nebula as I have demonstrated in a preceding number (February, 1908) of POPULAR ASTRONOMY is 13,400 degrees Fahrenheit or a trifle more than twenty-seven times the normal temperature of the atmospheric gases aforesaid, so that through the equation last set forth above, the density of that portion of the solar envelope may be regarded as practically in the same condition as the normal atmospheric envelope.

The maximum solar temperature of 13,400 degrees Fahrenheit is that of which I have denominated the "internal temperature" of the Sun, at which temperature there can be no chemical combination whatever as, if free from the compressive action of solar gravitation, all the molecules composed of these atoms would be disrupted, and the atoms exist and move as separate entities endowed merely with mass, motion and inertia as were the primordial components of the fundamental matter known as the luminiferous ether, and such combination can occur only at a level but slightly above the solar surface, where radiation to outside matter at a lower temperature (this matter being that of the planets and their satellites as well as of the comets and other widely diffused matter which is the residuum of the primitive solar nebula which has contracted to the present dimensions and conditions of the Sun) reduces the temperature of the surface matter to, and below, the degree at which these atoms by reason of the consequent decrease of their orbital velocity, fall into elliptical orbits around the focus within the molecule these ellipses having divers degrees of eccentricity varying from zero (in which case the atomic orbits are circular, the matter in this case constituting a gas in its simplest elemental form from the ether up to helium and hydrogen) up to infinity in which case the orbits become straight lines and the orbital revolutions longitudinal, or simple vibrations to and fro, under which conditions these atoms group themselves in divers combinations dependent upon their relative orbital velocities such combinations constituting what is known as solids. This grouping occurs primarily in, or near the surface of the photosphere where the solar matter is free to radiate its heat to the absorbent matter in surrounding space and to cool below the absolute temperature at which such chemical

combination becomes possible, this temperature in the case of such elements as carbon, silicon and calcium (which are most important and abundant components of the Earth as well of the Sun's surface matter) being not far below the effective temperature of the more or less solid radiating matter of the solar surface, which is 6700 degrees Fahrenheit according to my demonstration, or one half the maximum temperature which is that of the internal gases not far below the surface, or even intermingled with the finely divided solid matter of vastly higher thermal emissivity, which, grouped in more or less extensive aggregations such as the bright points and filaments of the photosphere, and the calcium and other flocculi above it, constitute the effective radiating matter of the Sun this being well depicted in the illustrations accompanying the paper in the last (November) number of POPULAR ASTRONOMY, by Professor William W. Payne on the subject of "Solar Vortices."

Saint Paul, Minnesota,

To be continued.

THE NORTHERN LIMIT OF THE ZODIACAL LIGHT.*

E. A. FATH.

This investigation was undertaken at the suggestion of Director Campbell in order to determine, if possible, the nature of a faint light which for years has been noticed in the summer along our northern horizon by several observers.

Three hypotheses as to the origin of this light seem to require examination: that it is an aurora; that it is twilight; and that it is the Zodiacal Light.

The observations were begun in July, 1907, but at that time consisted merely in noting whether the light was visible or not. They have been continued during the present summer upon a somewhat more extended scale, but have been made only at intervals when time could be spared from another investigation. In consequence the results are by no means as definite as one might wish. Then, too, the air has been much less clear than usual this summer owing to the forest fires, something less than fifty miles distant.

* Extracts from Lick Observatory Bulletin, No. 142.

The general appearance of the phenomenon when observed near midnight with a clear sky and no moon is that of a flat arch of light with its maximum of intensity near the north point of the horizon. At that time of night in the early part of July, 1908, the greatest altitude above the northern horizon was 18°. It extended westward about 40° and eastward to the Milky Way and was symmetrical with respect to the meridian. At the extremities it was only a degree or two above the horizon, depending upon the clearness of the air at such a low altitude. When, however, the observations were made before midnight the maximum was always to the west of the north point and after midnight always to the east.

When this much had been established it seemed advisable to note, as accurately as possible, the extent to the east and west of the north point, the altitude and azimuth of the maximum brightness and the greatest altitude to which it could be traced. This greatest altitude was always directly over the maximum. A simple instrument was designed and constructed for observing the horizontal and vertical angles referred to and set up on the roof of the observatory. To simplify matters the zero for azimuth was taken as the north point and azimuth recorded as east or west.

The method of observation was to locate the various parts of the luminous area with reference to the stars; then, pointing to these places, to read the circles to the nearest degree and note the sidereal time. Owing to the faintness of the light any one position might be in error by as much as 5°, but it is not likely that many are out so much as that. The instrument was set up on July 2, 1908, and used whenever possible.

1908	Mt. Ham. Sid. T.	Azimuth Eastern End	Azimuth Western End	Maximum Altitude	Maximum Azimuth	Greatest Altitude
June 27	$18^h\ 17^m$					
	19 47				15° E	
30	18 29					
July 2	18 00		41° W	10°	3 W	16°
	18 45		39 W	10	4 E	18
	19 45		30 W	9	13 E	16
	20 45	81° E	18 W	7	33 E	11
	21 15	81 E	8 W	7	35 E	10

* * * * *

The three hypotheses for the light as mentioned above will now be examined.

The aurora hypothesis does not seem tenable for three reasons:

1st.—The light in question is seen only in the summer, during

a period of about two months on either side of the summer solstice.

2d.—The spectroscopic evidence as given by the observations of July 29 and 31. The bright aurora line at λ 5571 could be seen in practically all parts of the sky and was no brighter when the spectroscope was pointed to the maximum of the luminous area than when pointed many degrees away and entirely outside this area. At this time there was no aurora visible as such. That the aurora line, λ 5571, can be seen when there is no visible aurora[1] may have been noted by Vogel[2] although he does not definitely state it. Campbell[3] seems to have been the first to call direct attention to this.

3d.—Columns 3 and 4 of the table show the maximum intensity to move with the Sun.

If the light were twilight it seems reasonable to suppose that the mean of the observations would show the maximum intensity to be on or very near the vertical circle passing through the Sun. Taking all the differences of azimuth from columns 3 and 4, the mean of the 29 observations shows the maximum to be 1°.7 farther east along the northern horizon than this vertical circle. Such a large difference does not appear accidental. Moreover the observations of July 2, $18^h\ 45^m$, shows that the light could be seen 46° north of the Sun, the Sun being at that time 30° below the horizon and thus 12° lower than the usually assigned limit for twilight. Observations[4] made during the summers of 1907 and 1908 to determine the zenith distance of the Sun at the end of twilight or the beginning of dawn all agree in making the zenith distance of the Sun 108°. The twilight hypothesis, therefore, does not explain the phenomenon satisfactorily.

Of the three hypotheses that of the Zodiacal Light remains.

The observations of July 19, 22, 23, 26, 29, August 27 and 28 show a direct connection between this light and the ordinary

[1] It seems advisable to call attention to what appears to be a mis-statement in Scheiner's *Spectralanalyse der Gestirne*. On page 343 it is stated "Auch Wright hat häufig die grüne Nordlichtlinie im Spectrum des Zodiakallichtes wahrgenommen, aber nur dann, wenn dieselbe auch an ausserhalb des Zodiakallichtes gelegenen Stellen auftrat, was zuweilen geschah, ohne dass ein Nordlicht direct zu sehen gewesen wäre." In Wright's report of his work on the spectrum of the Zoadiacal Light in *The American Journal of Science*, Series 3, 8, 45, 1874, occurs the statement, "On the other hand, the bright line was never seen when there was no aurora."

[2] "Die helle Linie erschien übrigens an den [three] genannten Tagen an allen Theilen des Himmels, der mit einem matten, lichten Schleier überzogen war, mehr oder weniger intensiv." *Astron. Nach.*, 27, 327, 1872.

[3] *Astrophysical Journal*, 2, 162, 1895.

[4] Not included in the observations taken above.

cone of the Zodiacal Light, a practically straight line forming the northern boundary of both the cone and the light under observation. This connection was also seen from the summit of Mt. Whitney, altitude 14,500 feet, on August 24. (See note at end of paper.) During the summer months the Zodiacal Light can be readily seen at Mt. Hamilton both in the west in the evening and in the east in the morning. The connection was seen with both the western and eastern cones.

The 1°.7 displacement of the maximum of the light to the east of the vertical circle passing through the Sun appears significant. If we assume the axis of greatest intensity of the Zodiacal Light to be in or near the ecliptic, then at midnight at the time of the summer solstice the axis will be perpendicular to the vertical circle passing through the Sun. Before midnight the western angle between the axis and the vertical circle will be less than 90° and after midnight the eastern angle will be less. In general, before the summer solstice the western angle will average less throughout the night than the eastern and vice versa. Now taking two points at the same altitude and symmetrically situated with respect to the vertical circle, the one to the west would average nearer the axis throughout the night for observations before the solstice and therefore be the brighter. In the same way, after the solstice, the eastern one would be the brighter. We might expect, therefore, that before the solstice the maximum observed intensity would be shifted toward the west and after the solstice toward the east, the shift varying with the distance of the Sun from the solstice. Now we find an observed displacement of 1°.7 toward the east and all the observations upon which this observed displacement depends were made after the solstice. An examination of the values in columns 3 and 4 of the table will also show a tendency for the displacement to increase with the time. The observations thus agree with the hypothesis.

The figures in the last two columns of the table aid in the determination of the apparent shape of the light observed. As stated above the figures give the distance from the Sun (in degrees) of the intersection with the ecliptic of the great circle drawn from the point of the greatest observed altitude through the point of greatest eastern or western elongation. The total range of intersections is from 40° to 105°. When the results of the last four columns of the table are plotted, using the Sun as origin and the ecliptic as the axis of abscissas it is seen that the light observed is distributed fairly symmetrically with respect to

the Sun. It passes 46° to the north and intersects the ecliptic about 70° on either side. This applies, of course, only to the part north of the ecliptic. Assuming symmetry with respect to the ecliptic also we have the lenticular appearance of the Zodiacal Light. The effect of atmospheric absorption would be to bring the intersections too close to the Sun. The apex of the Zodiacal Light cone can be seen at various distances from the Sun ranging from 40° to over 100°. This agreement between the observed position of the apex and the position of the intersections is as satisfactory as can be expected from the nature of the objects observed.

The observations of August 28 and September 3 together with the results of others made after the reduction of the observations given above lead to the following table of results. The first column gives the distance of the apex of the Zodiacal Light cone from the Sun; the second column the angle made by the north side of the cone with the ecliptic; the last column the latitude of the intersection of the prolongation of the northern boundary of the cone with the great circle passing through the Sun perpendicular to the ecliptic.

		Apex from Sun	Angle with Ecliptic	Latitude
August	28	47°	47°	38°
September	3	45	56	46
	24	76	45	44
	25	81	42	41
	26	80	41	41
	27	80	39	39
	28	79	38	38
	29	82	31	31[5]
	30	81	39	39
October	1	82	39	39
	3	83	46	46
	4	84	55	55
	5	88	39	39
	6	88	45	45

The mean of the results in the last column shows that we should expect the Zodiacal Light to extend 42° north of the Sun if we neglect curvature of the boundary and the effect of atmospheric absorption. The former would decrease and the latter increase the distance. Observations above the northern horizon give 46°.

In the classic volume of observations of the Zodiacal Light by Jones[6] I find that by extending the boundaries of the "diffuse" light they would in many cases exceed latitudes of 45° both north and south of the Sun. The writer has not at present the time

[5] Given half weight in mean on account of conditions under which the observations were made.

[6] *Observations of the Zoadiacal Light*. Vol. 3 of the Report of the United States Japan Expedition, 1856.

necessary to investigate carefully this great wealth of material and therefore desires only to call attention to the agreement between the observations of Jones and his own.

From the evidence at hand it seems fair to conclude that the phenomenon observed is the Zodiacal Light, and probably the same phenomenon as seen by Herrick,[7] Newcomb,[8] and Barnard.[9] For the sake of completeness the observations should extend over another summer, say from May 1 to August 10. It could then be determined whether the maximum is to the west of the vertical circle passing through the Sun before the solstice and to the east after the solstice. It seems best, however, to publish these preliminary results for the benefit of any who may be interested in the matter of making the necessary observations next summer. It would also be of great value to have observations made at stations in the southern hemisphere from the first of November to the end of January.

In this paper it has been assumed that the axis of the Zodiacal Light is in or very near the ecliptic, in accordance with many observations. Some recent work places the axis in the plane of the Sun's equator. This would materially change the conclusions reached. Mention has also been made of the apex of the Zodiacal Light cone. This is taken as the point of intersection of the two sides of the cone. No real apex is seen at Mt. Hamilton as the cone near the "apex" merges directly into the zodiacal band which can usually be traced across the entire sky.

In conclusion, I wish to acknowledge my indebtedness to Director Campbell for his coöperation and suggestions, and especially for the trouble taken to secure the Mt. Whitney observations under trying circumstances. It is also a pleasure to acknowledge some suggestions from Mr. W. H. Wright, Astronomer in the Lick Observatory.

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[7] *Silliman's Jour. of Science*, 39, 331, 1840.
[8] *Astrophysical Journal*, 22, 209, 1905.
[9] *Ibid.*, 23, 168, 1906.

NOTE.—Director Campbell spent the night of August 24, 1908, on the summit of Mt. Whitney, altitude 14,500 feet, latitude $+36^\circ$ 35′. At the request of the writer he kindly undertook some observations of the Zodiacal Light; it being arranged that the writer should make simultaneous observations at Mt. Hamilton. The Mt. Whitney observations are as follows:

"At 8^h 15^m Pacific Standard Time the Zodiacal Light could be faintly seen in the west. It extended along the ecliptic and then around the northwest horizon almost to the north point. Under β *Urs. Maj.* its altitude was $\frac{1}{3}$ that of β. At midnight nothing could be seen along the north horizon. Air exceptionally clear."

The observations at Mt. Hamilton showed nothing further. The smoke from the forest fires doubtless obscured the faint light.

JOHN KROM REES.*

HAROLD JACOBY.

It is the custom to mark the passing of a well-known man with a short notice of biography; and it is not difficult to recite a list of services, enumerate honors and distinctions conferred by public bodies, or recapitulate scientific researches and publications. But to the writer these are cold and hard when said of Rees; to him Rees was known best as a friend—that rare friendship of which the beginning is outside the grasp of memory, the end a green sod.

JOHN KROM REES
Died March 9, 1907

Surely, if there exists a relation adapted better than any other to make one acquainted with the good or bad in any man, it is the relation of a subordinate to his chief. During eighteen long years the writer sat at his work under Rees: in all that time there never came down to him an unkind word; never once did a serious difference of opinion arise. In eighteen years one becomes accustomed to any man; the few like Rees one comes to love. These words will fail signally in their purpose, if they do not convey to his sorrowing family such poor consolation as may come from those who feel and suffer with them.

Rees was but fifty-five on his last birthday; in his short life he had served Washington University as a professor five years, and Columbia University twenty-one. He had been president of the New York Academy of Sciences two years, and secretary of the American Metrological Society fourteen years. For six years he was secretary of the Columbia University Council. He was a fellow of the Royal Astronomical Society of London, a member of the Astronomische Gesellschaft of Leipzig, and in 1901 was

* *School of Mines Quarterly*, Vol. XXVIII, No. 4.

created a chevalier of the Legion d'Honneur in recognition of his services as one of the judges at the Paris exposition in that year. His principal observational research was a study of the "Variation of Terrestrial Latitudes and the Aberration of Light," made at Columbia University, in coöperation with the Royal Observatory, Naples. This was the first application of the method of simultaneous observations at two stations situated on the same parallel of latitude, but separated widely in longitude. The work was continued from 1893 to 1900: the method has since come into general use: and the International Geodetic Association, which includes all civilized governments, has now established four permanent stations to carry it on.

It was also during Rees's administration that the astronomical department of Columbia University undertook the publication of Rutherfurd's valuable series of star photographs. Through his efforts this enterprise was made possible; he took a keen interest in it, and spared no pains to further the work during a long series of years.

In educational matters Rees was at all times most active. His public lectures were frequent. Characterized especially by lucidity, they always attracted large audiences; people came to hear him again and again. But his most lasting contribution to educational development was his establishment of the Columbia summer school of geodesy. It is probable that he was the first to recognize practical field work in this subject as an indispensable adjunct in the training of civil engineers. Here he was at his best: his point of view always that of the genuine man of science, seeking ever the truth for its own sake; never exalting mere technique at the expense of theoretic perfection; never limiting his exposition of a subject to the side having most value from the financial point of view. Students frequently came back to him in later years; they always spoke of his summer school as the most agreeable memory of their college years. Whenever this occurred, he was a happy man for days.

The following letter from Professor F. R. Hutton shows well the cordial relations that existed between Rees and his professional associates in the days of old Columbia :

"Rees entered the School of Mines in 1872 on graduating from Columbia College. His class was of Rölker, Weller, Leavens and Wright, who have all risen to high standing in their respective specialities. Chemistry figured very largely in the old course of study under Bolton, Waller and Kerns, whom all delight to remember whose memory runs back to those old times.

The buildings were most inadequate and poorly ventilated, but they hummed with a spirit of busy and ever enthusiastic industry. On graduation Rees went at once to his work at Washington University, St. Louis, where he took a very earnest and vital interest in all matters relating to the university. It was while he lived and labored there that the memorable fire occurred in the Southern hotel, from which Rees and his young wife escaped at peril of their lives and lost all their belongings. They descended from windows on ropes.

"Coming to Columbia as assistant to Professor William Guy Peck, Rees had to begin with very small facilities. A wooden structure on the campus, with a small refracting telescope and a time instrument, were all he had, but he rendered most effective service with this apparatus, not only in descriptive astronomy with college students, but in effective coöperation with the schools of the city, where classes were formed for evening visits to the little observatory, sadly interfering with Rees's dear ambition to do real scientific work. But he felt this an obligation he could not slight. It was a great gain when the new buildings and the library at 49th street were erected, giving at least a mechanical elevation above the horizon of the surrounding roofs; but difficulties from smoke and railroad vibrations, caused by heavier engines and increasing weight and number of trains, all stood in the way of any ambition to rival research observatories. It was a dear dream of his to establish a great observatory on the Hudson at some point remote from the interference of metropolitan conditions.

"On the coming of Professor Trowbridge in 1878 the need of geodetic surveying was at once considered and the courses in geodesy for civil engineers established to meet it. These were also made to furnish necessary preliminary training for the summer school with which Rees was so prominently identified. The first schools were held by him at Cooperstown, N. Y,, and continued there until the transfer to Sunapee, N. H., from which place they moved to their present location at Osterville, Mass. The bond between Professors Rees and Peck was a very close and friendly one both during the period of Rees's service as adjunct professor, and later when he was made full professor over the independent department of astronomy."

At times Rees was persuaded to go beyond his quiet field of activity in the university. His most lasting public service to the nation was rendered as secretary of the Metrological Society, in furthering the introduction of standard or railroad time.

The late Dr. F. A. P. Barnard, president of Columbia College, made the following reference to Professor Rees in his testament, dated 1886:

"I give to my friend, Professor John Krom Rees, the watch known as my Cosmic Time Watch, as a mark of my regard and of my appreciation of his zealous efforts for the promotion of metrological reform and for the introduction of the now established system of public standard time."

Rees's attitude towards the scientific work of others was one of extraordinary modesty. To him the past masters of astronomy were not men; they were demi-gods, to be mentioned in respectful accents and with lowered tones. Even living visible masters of the craft commanded from him a degree of respect such as he could not have offered even to crowned royalties. His own work might be as good as theirs, but he could never see it so.

Another marked characteristic was his extreme delight when visited by any one to whom it was possible to do a favor. This was his pleasure. No trouble was too great or time-consuming; no return, not even thanks, was expected. No man ever had fewer enemies; his friends equaled in number those who knew him. When at last the heavy weight of disease was laid upon him he met it as a man should. No querulous repining; regret only that his work must stop; his solace that others would carry the good work on. Mother earth, that he loved well to measure and compute, will give him sleep; to him the peaceful end is surely a release.

PLANET NOTES FOR JANUARY, 1909.

Mercury will be in conjunction with Uranus on Jan. 1, both planets being then so nearly behind the Sun as to be invisible. Mercury will be at greatest elongation, east from the Sun 18° 26°, on the evening of Jan. 26. This will be a favorable time to see the planet, since it will be relatively bright and well up from the horizon at sunset. It may be caught sight of toward the west, probably within a half hour after sunset.

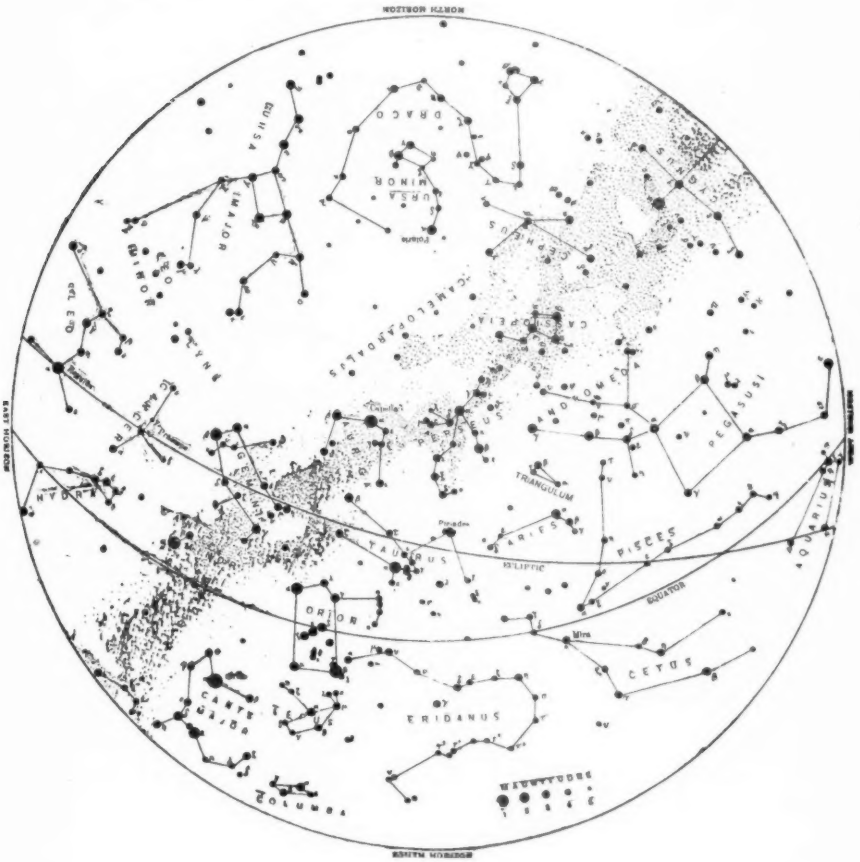
Venus may be seen toward the southeast in the morning for an hour or more before sunrise. Venus and Uranus will be in conjunction on Jan. 30, Uranus being at nearest point 21′ south of Venus.

Mars may seen toward the southeast for about three hours before sunrise. Its altitude is low at best and its apparent diameter small, so the time is not favorable for the study of this planet.

Jupiter is near the meridian, at a good high altitude, at four o'clock in the morning. The planet is in good position for study with the telescope during all of the latter half of the night. Jupiter is in the constellation Leo about fifteen degrees southeast from Regulus and is much brighter than Regulus.

Saturn is near the meridian at sunset and so may be studied in the early evening. Saturn is in Aquarius and will move northeastward into Pisces.

Uranus will be in conjunction, behind the Sun, Jan. 7 and so cannot be studied during this month.



THE CONSTELLATIONS AT 9:00 P. M., JANUARY 1, 1909.

Neptune will be at opposition on the morning of Jan. 6. During this month it may be observed at all hours of the night. The planet is in the constellation Gemini a little south and west from the star δ.

COMET NOTES.

New Ephemeris of the Comet Tempel$_3$-Swift.—In A.N. 4277 Mr. E. Maubant gives corrected ephemerides of this periodic comet extending from Oct. 20 to Dec. 31. The portion for December is given below. Mr. Maubant remarks that in order to represent the observation of Mr. Javelle at Nice on Sept. 29, it is necessary to diminish the mean daily motion of the comet by 0″.38. The correction, he thinks, is too large to be accounted for by inaccuracies in the computation of the perturbations, and he calls attention to the fact that Bossert found it necessary to apply a similar correction to the calculations for the apparition of 1891. It is possible that this comet is subject to a retardation such as Schulhof found for the comet Tempel$_3$ and Lamp for Brorsen's comet.

EPHEMERIS OF COMET TEMPEL$_3$-SWIFT.

[For Paris midnight; perihelion 1908 Oct. 4.528].

Date	α h	α m	α s	δ	log r	log Δ
Dec. 1	9	31	33	+17° 27′.3		
2		32	07	17 16.5		
3		32	38	17 06.1	0.1381	9 8214
4		33	05	16 53.9		
5		33	28	16 45.9		
6		33	47	16 36.3		
7		34	03	16 27.0	0.1463	9.8215
8		34	15	16 17.9		
9		34	23	16 09.1		
10		24	28	16 00.6		
11		34	29	15 52.4	0.1547	9.8218
12		34	26	15 44.5		
13		34	29	15 36.9		
14		34	10	15 29.5		
15		33	56	15 22.5	0.1631	9.8224
16		33	39	15 15.7		
17		33	18	15 09.3		
18		32	54	15 03.1		
19		32	26	14 57.3	0.1715	9.8236
20		31	54	14 51.7		
21		31	19	14 46.4		
22		30	41	14 41.4		
23		30	00	14 36.6	0.1799	9.8156
24		29	15	14 32.2		
25		28	27	14 28.0		
26		27	36	14 24.1		
27		26	43	14 20.4	0.1884	9.8286
28		25	46	14 17.0		
29		24	47	14 13.9		
30		23	45	14 11.0		
31	9	22	40	+14 08.4	0.1968	9.8328

Comet Morehouse.—Professor E. B. Frost, Director of the Yerkes Observatory, calls attention to the recent increase of brightness of Morehouse's Comet and writes on October 29:—

"It was visible to the naked eye, and three or four degrees of tail could readily be seen in a small field glass. Three spectrum plates were obtained with the Zeiss ultra-violet doublet and objective prism by Mr. Parkhurst with some assistance from me. Two of these had exposures of one hour. No continuous spectrum was perceptible, whence we may reach the important in-

ference that last night the comet's light was very largely intrinsic. Seven bands were very conspicuous as knots on the plate. I am measuring the spectra this morning, but have no doubt that they will prove to show the ordinary hydrocarbon spectrum."

The photographs taken last night at the Harvard Observatory, show a tail at least nine degrees in length, and much longer than on previous nights.

Astronomical Bulletin, No. 343, EDWARD C. PICKERING.

Harvard College Observatory, Oct. 31, 1908.

Approximate Ephemeris of Comet *c* 1908.—The following approximate ephemeris of Comet *c*, computed by Dr. Smart, is taken from *The Observatory* for November, 1908. It shows that the comet will move steadily southward during the winter reaching the south polar regions of the sky during March. During the first half of January it will be invisible because of its being behind the Sun.

EPHEMERIS OF COMET *c* 1908.

Greenwich Midnight	R. A.	Decl.	Greenwich Midnight	R. A.	Decl.
Dec. 11	$18^h\ 50^m\ 0^s$	$-12°\ 56'$	Mar. 8	$18^h\ 10^m\ 00^s$	$-65°\ 49'$
15	18 50 0	−15 06	12	17 49 12	−69 59
28	18 49 55	−21 34	16	17 14 20	−74 02
1909			20	16 12 44	−77 37
Jan. 15	18 49 20	−30 08	24	14 30 16	−79 38
19	18 49 08	−32 05	28	12 31 00	−78 55
23	18 48 56	−34 07	Apr. 1	11 06 08	−75 45
27	18 48 40	−36 11	5	10 17 52	−71 27
31	18 48 20	−38 20	9	9 50 08	−66 47
Feb. 4	18 47 48	−40 38	13	9 35 52	−62 06
8	18 46 52	−43 04	17	9 23 52	−57 38
12	28 45 36	−45 40	21	9 16 40	−53 21
16	18 43 44	−48 26	25	9 13 20	−49 30
20	18 41 04	−51 24	29	9 11 08	−46 08
24	18 37 12	−54 36	May 3	9 09 56	−42 59
28	18 31 52	−58 06	7	9 09 40	−40 10
Mar. 4	18 23 20	−61 50	11	9 10 04	−37 38

Brightness 3.1 on Feb. 1, 3.0 on March 1, 2.4 on April 1 and 0.9 on May 1.

Comet *c* 1908 (Morehouse).—This comet continues to exhibit wonderful changes from night to night. On Oct. 28 the writer noted it for the first time as visible to the naked eye. On this night only the head of the comet and a very little of the tail could be seen, but on Oct. 29 it was quite conspicuous and the tail could be traced for about 6° length with the unaided eye. On Oct. 30 the tail was fainter, scarcely visible to the eye at all. On Nov. 13 and 17 the head of the comet was scarcely visible, and the tail not at all, to the naked eye, but on Nov. 20 the head was quite bright and the tail could be caught by averted vision for a length of four or five degrees. The photographs on these dates show that the tail was from twelve to fifteen degrees long and full of marvelous details of structure.

The following ephemeris for the first days of December is taken from *A. N.* 4276.

	R. A.	Decl.	log *r*	log Δ	Brightness
Nov. 29	$18^h\ 50^m\ 24^s$	$-5°\ 31'.9$	0.0217	0.2068	4.52
30	50 22	−6 12.6			
Dec. 1	50 21	−6 52.5			
2	18 50 19	−7 31.6			

		h	R. A.	Decl.	log r	log Δ	Brightness
Dec.	3	18	50^m 18^s	−8° 10′.0	0.0098	0.2230	4.43
	4		50 17	−8 47 .7			
	5		50 16	−9 24 .8			
	6		50 14	−10 01 .2			
	7	18	50 13	−10 36 .9	9.9992	0.2375	4.35

H. C. WILSON.

Comet of 69 B. C. and Donati's Comet.—In *A. N.* 4277 Mr. H. H. Kritzinger gives the results of his investigations in regard to the identity of Donati's comet of 1858 with one observed by the Chinese in the year 69 B. C. He reaches a negative conclusion in regard to the comet of 69 B. C. but finds that another great comet observed by the Chinese in the year—146 (147 B. C.) might possibly be identified with Donati's comet. The elements computed for Donati's comet admit of a period all the way from 1879 to 2040 years.

VARIABLE STARS.

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Greenwich Mean Time beginning with noon. To reduce to Central Standard time subtract 6 hours, or for Eastern time subtract 5 hours.]

136.1907 Andr.

	d	h
Jan.	12	17

U Cephei

	d	h
Jan.	1	1
	3	13
	6	1
	8	13
	11	1
	13	13
	16	0
	18	12
	21	0
	23	12
	26	0
	28	12
	30	23

Z Persei

	d	h
Jan.	1	4
	4	5
	7	6
	10	8
	13	9
	16	10
	19	12
	22	13
	25	15
	28	16
	31	17

RY Persei

	d	h
Jan.	5	20
	12	17
	19	13
	26	10

RZ Cassiop.

	d	h
Jan.	1	1
	2	6
	3	10
	4	15
	5	20
	7	0
	8	5
	9	10
	10	15
	11	19
	13	0
	14	5
	15	9
	16	14
	17	19
	18	23
	20	4
	21	9
	22	13
	23	18
	24	23
	26	3
	27	8
	28	13
	29	17
	30	22

RX Cephei

	d	h
Jan.	12	1

Algol

	d	h
Jan.	3	7
	6	4
	9	1
	11	22
	14	19

Algol

	d	h
Jan.	17	16
	20	12
	23	9
	26	6
	29	3

RT Persei

	d	h
Jan.	1	11
	2	7
	3	3
	4	0
	4	20
	5	17
	6	13
	7	9
	8	6
	9	2
	9	23
	10	19
	11	15
	12	12
	13	8
	14	4
	15	1
	15	21
	16	18
	17	14
	18	10
	19	7
	20	3
	21	0
	21	20
	22	16
	23	13
	24	9

RT Persei

	d	h
Jan.	25	5
	26	2
	26	22
	27	19
	28	15
	29	11
	30	8
	31	4

λ Tauri

	d	h
Jan.	1	20
	5	19
	9	18
	13	16
	17	15
	21	14
	25	13
	29	12

RW Tauri

	d	h
Jan.	1	18
	4	12
	7	7
	10	1
	12	20
	15	14
	18	8
	21	3
	23	21
	26	16
	29	10

RV Persei

	d	h
Jan.	2	20
	4	19
	6	18
	8	18

RV Persei

	d	h
Jan.	10	17
	12	17
	14	16
	16	15
	18	15
	20	14
	22	13
	24	13
	26	12
	28	11
	30	11

RW Persei

	d	h
Jan.	8	11
	21	16

RS Cephei

	d	h
Jan.	13	9
	25	19

RY Aurigæ

	d	h
Jan.	3	2
	5	19
	8	13
	11	6
	14	0
	16	17
	19	11
	22	4
	24	22
	27	15
	30	9

RZ Aurigæ

	d	h
Jan.	1	20
	4	21
	7	21
	10	21

RZ Aurigae

d	h
13	22
16	22
19	23
22	23
25	23
29	0

RW Geminorum

Jan.	1	10
	4	6
	7	3
	10	0
	12	21
	15	17
	18	14
	21	11
	24	8
	27	5
	30	1

RW Monoc.

Jan.	1	0
	2	22
	4	20
	6	18
	8	15
	10	13
	12	11
	14	9
	16	6
	18	4
	20	2
	22	0
	23	21
	25	19
	27	17
	29	15
	31	12

RU Monoc.

Jan.	1	4
	2	1
	2	23
	3	20
	4	18
	5	15
	6	13
	7	10
	8	8
	9	5
	10	3
	11	0
	11	22
	12	19
	13	17
	14	14
	15	12
	16	9
	17	7
	18	5
	19	2
	20	0
	20	21
	21	19
	22	16

RU Monoc.

d	h
23	14
24	11
25	9
26	6
27	4
28	1
28	23
29	20
30	18
31	15

R Canis Maj.

Jan.	1	3
	2	7
	3	10
	4	13
	5	16
	6	20
	7	23
	8	7
	9	2
	10	5
	11	9
	12	12
	13	15
	14	18
	15	22
	17	1
	18	4
	19	8
	20	11
	21	14
	22	17
	23	21
	25	0
	26	3
	27	6
	28	10
	29	13
	30	16
	31	19

Y Camelop.

Jan.	2	13
	5	20
	9	3
	12	11
	15	18
	19	1
	22	9
	25	16
	28	23

RR Puppis

Jan.	6	22
	13	8
	19	19
	26	5

V Puppis

Jan.	1	8
	2	19
	4	6
	5	17
	7	3
	8	14

V Puppis

d	h
Jan. 10	1
11	12
12	23
14	10
15	21
17	8
18	19
20	6
21	17
23	3
24	14
26	1
27	12
28	23
30	10

X Carinæ

Jan.	2	2
	3	4
	4	6
	5	8
	6	10
	7	12
	8	14
	9	16
	10	18
	11	20
	12	22
	14	0
	15	2
	16	4
	17	6
	18	8
	19	10
	20	12
	21	14
	22	16
	23	18
	24	20
	25	22
	27	0
	28	2
	29	3
	30	5
	31	7

S Cancri

Jan.	3	0
	12	11
	21	23
	31	11

S Velorum

Jan.	1	9
	7	7
	13	5
	19	4
	25	2
	31	1

RR Velorum

d	h
Jan. 2	8
4	4
6	1
7	21
9	18
11	14
13	11
15	7
17	4
19	0
20	21
22	17
24	14
26	10
28	7
30	3

SS Carinæ

Jan.	2	9
	5	17
	9	0
	12	7
	15	14
	18	21
	22	5
	25	12
	28	19

Z Draconis

Jan.	1	12
	2	22
	4	6
	5	15
	6	23
	8	8
	9	16
	11	1
	12	10
	13	18
	15	3
	16	11
	17	20
	19	4
	20	13
	21	22
	23	6
	24	15
	25	23
	27	8
	28	16
	30	1
	31	10

SS Centauri

Jan.	2	0
	4	11
	6	23

SS Centauri

d	h
	10
Jan. 9	22
11	9
14	21
16	8
19	
21	20
24	7
26	19
29	5
31	17

δ Libræ

Jan.	2	9
	4	16
	7	0
	9	8
	11	16
	14	0
	16	8
	18	16
	20	23
	23	7
	25	15
	27	23
	30	7

U Coronæ

Jan.	1	10
	4	20
	8	7
	11	18
	15	5
	18	16
	22	3
	25	13
	29	0

R Aræ

Jan.	1	18
	5	4
	9	14
	14	0
	18	10
	22	21
	27	7

UZ Cygni

Jan.	3	15

143.1907 Andr.

Jan.	3	11
	6	5
	8	23
	11	18
	14	12
	17	6
	20	1
	22	19
	25	13
	28	8
	31	2

Maxima of Variable Stars of Short Period not of the Algol Type.

Unless otherwise indicated the times of maxima only are given; and the times of minima may be found by subtracting the interval printed in parentheses under the names of the stars.

104.1907

Cassiopeiæ

	d	h
Jan.	3	10
	7	12
	11	14
	15	16
	19	17
	23	19
	27	21
	31	22

RW Cassiop.

(−5 19)

Jan.	15	12
	28	7

RX Aurigæ

(−4 0)

Jan.	8	13
	20	4
	31	19

Y Aurigæ

(−0 18)

Jan.	4	1
	7	21
	11	18
	15	15
	19	11
	23	8
	27	4
	31	1

T Monoc.

(−9 23)

Jan.	24	13

W Geminorum

(−2 2)

Jan.	1	20
	9	18
	17	16
	25	14

ζ Geminorum

(−5 0)

Jan.	9	21
	20	1
	30	5

RU Camelop.

(−9 12)

Jan.	3	9

RU Camleop.

	d	h
Jan.	25	15

V Carinæ

(−2 4)

Jan.	7	2
	13	18
	20	11
	27	4

T Velorum

(−1 10)

Jan.	4	23
	9	15
	14	6
	18	21
	23	13
	28	4

W Carinæ

(−1 0)

Jan.	5	22
	10	7
	14	16
	19	1
	23	10
	27	19

S Muscæ

(−3 11)

Jan.	5	13
	15	5
	24	21

T Crucis

(−2 2)

Jan.	6	15
	20	2
	26	20

R Crucis

(−1 10)

Jan.	5	15
	11	10
	17	6
	23	2
	28	22

S Crucis

(−1 12)

Jan.	2	20
	7	13
	12	5
	16	22

S Crucis

	d	h
Jan.	21	14
	26	7
	31	0

RZ Centauri

Jan.	1	11
	2	9
	3	8
	4	6
	5	5
	6	3
	7	2
	8	0
	8	23
	9	21
	10	20
	11	18
	12	17
	13	15
	14	14
	15	12
	16	11
	17	9
	18	8
	19	7
	20	5
	21	4
	22	2
	23	1
	23	23
	24	22
	25	20
	26	19
	27	17
	28	16
	29	14
	30	13
	31	11

W Virginis

(−8 5)

Jan.	14	8
	31	15

V Centauri

(−1 11)

Jan.	3	21

V Centauri

	d	h
Jan.	9	9
	14	21
	20	9
	25	21
	31	9

R Triang. Austr.

(−1 0)

Jan.	2	9
	5	18
	9	4
	12	13
	15	22
	19	8
	22	17
	26	3
	29	12

S Triang. Austr.

(−2 2)

Jan.	4	10
	10	18
	17	2
	23	9
	29	17

S Normæ

(−4 10)

Jan.	9	1
	18	19
	28	13

RV Scorpii

(−1 10)

Jan.	4	18
	10	20
	16	21
	22	23
	29	0

Y Lacertae

(−1 10)

Jan.	3	14
	7	21
	12	5
	16	12
	20	20
	25	4
	29	11

δ Cephei

	h	d
	(−1	10)
Jan.	6	6
	11	15
	16	23
	22	8
	27	17

V Lacertae

(−1 16)

Jan.	4	1
	9	1
	14	0
	19	0
	23	23
	28	23

X Lacertae

Minimum

Jan.	3	7
	9	3
	14	14
	20	1
	25	11
	30	24

SW Cassiop.

Jan.	6	3
	11	16
	17	5
	22	18
	28	7

RS Cassiop.

(−1 19)

Jan.	1	12
	7	19
	14	2
	20	9
	26	17

RY Cassiop.

(−7 10)

Jan.	6	18
	18	21
	31	1

Approximate Magnitudes of Variable Stars on November 1, 1908.

[Communicated by the Director of Harvard College Observatory, Cambridge, Mass.]

Name.	R. A. 1900. h	m	Decl. 1900. °	′	Magn.	Name.	R.A. 1900. h	m	Decl. 1900. °	′	Magn.
X Androm.	0	10.8	+46	27	12.0*d*	Y Cephei	0	31.3	+79	48	9.2
T Androm.		17.2	+26	26	9.6 *i*	U Cassiop.		40.8	+47	43	11.6 *i*
T Cassiop.		17.8	+55	14	8.0	V Androm.		44.6	+35	6	11.2 *i*
R Androm.		18.8	+38	1	8.9*d*	RR Androm.		45.9	+33	50	12.5*d*
S Ceti		19.0	− 9	53	10.0 *i*	W Cassiop.		49.0	+58	1	8.6

Approximate Magnitudes of Variable Stars on Nov. 1, 1908—Con.

Name.	h	R. A. 1900. m	Decl. 1900. °	′	Magn.	Name.	h	R. A 1900 m	Decl. 1900 °	′	Magn.
U Androm.	1	9.8	+40	11	<12	RS Gemin.	6	55.2	+30	40	12.2*d*
S Piscium		12.4	+ 8	24	<12.0	V Can. Min.	7	1.5	+ 9	2	8.9
S Cassiop.		12.3	+72	5	11.0*d*	R Gemin.		1.3	+22	52	8.9
U Piscium		17.7	+12	21	11.4 *i*	R Can. Min.		3.2	+10	11	9.7 *i*
R Piscium		25.5	+ 2	22	<12.5	RR Monoc.		12.4	+ 1	17	10.2 *i*
RU Androm.		32.8	+38	10	<12.5	V Gemin.		17.6	+13	17	9.0
Y Androm.		33.7	+38	50	<12.4	S Can. Min.		27.3	+ 8	32	9.6*d*
X Cassiop.		49.8	+58	46	10.6 *i*	T Can. Min.		28.4	+11	58	9.2 *i*
U Persei		53.0	+54	20	9.0 *i*	Z Puppis		28.3	−20	27	<12
R Arietis	2	10.4	+24	35	<12	U Can. Min.		35.9	+ 8	37	11.5 *i*
W Androm.		11.2	+43	50	10.0*d*	S Gemin.		37.0	+23	41	<12.5
Z Cephei		12.8	+81	13	<14	T Gemin.		43.3	+23	59	9.0
o Ceti		14.3	− 3	26	3.8*d*	U Puppis		56.1	−12	34	11.6
S Persei		15.7	+58	8	8.8	R Cancri	8	11.0	+12	2	9.3*d*
R Ceti		20.9	− 0	38	80. *i*	V Cancri		16.0	+17	36	8.6 *i*
U Ceti		28.9	−13	35	7.3	RT Hydrae		24.7	− 5	59	8.4 *i*
RR Cephei		30.4	+80	42	10.5*d*	U Cancri		30.0	+19	14	<12.5
R Trianguli		31.0	+33	50	9 0 *i*	X Urs. Maj.		33.7	+50	30	<12
T Arietis		42.8	+17	6	9.3*d*	S Hydrae		48.4	+ 3	27	9.4 *i*
W Persei		43.2	+56	34	9.4*d*	T Hydrae		50.8	− 8	46	8.5 *i*
U Arietis	3	5.5	+14	25	<12	T Cancri		51.0	+20	14	8.5 *i*
X Ceti		14.3	− 1	26	10.7*d*	W Cancri	9	4.0	+25	39	9.3 *i*
Y Persei		20.9	+43	50	8.6 *i*	X Hydrae		30.7	−14	15	10.0
R Persei		23 7	+35	20	9.5 *i*	Y Draconis		31.1	+78	18	14.0*d*
Nov. Per. 2		24.4	+43	34	12.6	R Leo. Min.		39.6	+34	58	8.3 *i*
S Fornacis		41.9	−24	42	8.5	RR Hydrae		40.4	−23	34	10.8
T Tauri		16.2	+19	18	9.8	R Leonis		42.2	+11	54	6.0 *i*
W Eridani	4	7 3	−25	24	<12	Y Hydrae		46.4	−22	33	7.3
R Tauri		22.8	+ 9	56	<12	V Leonis		54.5	+21	44	12.4*d*
W Tauri		22.2	+15	49	9.0	R Urs. Maj.	10	37.6	+69	18	9.8*d*
S Tauri		23.7	+ 9	44	<12	W Leonis		48.4	−14	15	<12.5
T Camelop.		30.4	+65	57	12.5	S Leonis	11	5.7	+ 6	0	<12.5
RX Tauri		32.8	+ 8	9	<12	R Comae		59.1	+19	20	<12.5
X Camelop.		32.6	+74	56	9.6 *i*	SS Virginis	12	20.1	+ 1	19	8.0
V Tauri		46.2	+17	22	<12.5	T Can. Ven.		25.2	+32	3	11.8
R Orionis		53.6	+ 7	59	<12.0	T Urs. Maj.		31.8	+60	2	9.8*d*
R Leporis		55.0	−14	57	7.9	RS Urs. Maj.		34.4	+59	2	10.0 *i*
V Orionis	5	0.8	+ 3	58	12.2*d*	S Urs. Maj.		39.6	+61	38	8.4 *i*
T Leporis		0.6	−22	2	8.4	T Urs. Min.	13	32.6	+73	56	9.0 *i*
R Aurigae		9.2	+53	28	9.0 *i*	R Can. Ven.		44.6	+40	2	8.8 *i*
S Aurigae		20.5	+34	4	8.6 *i*	U Urs. Min.	14	15.1	+67	15	8.7 *i*
W Aurigae		20.1	+36	49	9.0 *i*	S Bootis		19.5	+54	16	12.0 *i*
S Orionis		24.1	− 4	46	8.0 *i*	V Bootis		25.7	+39	18	8.1
T Orionis		30.9	− 5	32	10.5 *i*	R Camelop.		25.1	+84	17	8.2 *i*
S Camelop.		30.2	+68	45	10.2*d*	R Bootis		32.8	+27	10	10.0*d*
RR Tauri		33.3	+26	19	11.1*d*	S Coronae	15	17.3	+31	44	12.4
U Aurigae		35.6	+31	59	9.5	S Ur-. Min.		33.4	+78	58	9.6 *i*
RZ Aurigæ		42.9	+31	39	11.0	R Coronae		44.4	+28	28	6.0
U Orionis		49.9	+20	10	10.6*d*	X Coronae		45.2	+36	35	11.2*d*
— Orionis		50.0	+ 20	9	10.0	V Coronae		46.0	+39	52	11.8*d*
Z Aurigæ		53.6	+53	18	10.1 *i*	Z Coronae		52.2	+29	32	11.0*d*
X Aurigae	6	4.4	+50	15	8.5	RR Herculis	16	1.5	+50	46	8.6
SS Aurigae		5.6	+47	47	14.0*d*	U Serpentis		2.5	+10	12	13.0*d*
V Aurigae		16.5	+47	45	11.0 *i*	RU Herculis		6.0	+25	20	8.0
V Monoc.		17.7	− 2	9	9.6*d*	W Coronae		11.8	+38	3	10.4*d*
S Lyncis		35.9	+58	0	12.2*d*	U Herculis		21.4	+19	7	9.3*d*
X Gemin.		40.7	+30	23	11.0*d*	SS Herculis		28.0	+ 7	3	10.5*d*
W Monoc.		47.5	− 7	2	10.0	W Herculis		31.7	+37	32	10.6*d*
Y Monoc.		51.3	+11	22	11.0 *i*	R Draconis		32.4	+66	58	7.3 *i*
X Monoc.		52.4	− 8	56	13.5	S Herculis		47.4	+15	7	7.5
R Lyncis		53.0	+55	28	9.6 *i*	RV Herculis		56.8	+31	22	10.3

Approximate Magnitudes of Variable Stars on Nov. 1, 1908—Con.

Name.	R. A. 1900.	Decl. 1900	Magn.	Name.	R. A. 1900.	Decl. 1900	Magn.
	h m	° ′			h m	° ′	
Z Ophiuchi	17 14.5	+ 1 37	8.8*d*	WX Cygni	20 14.8	+37 8	10.6 *i*
RS Herculis	17.5	+23 1	11.2*d*	U Cygni	16.5	+47 35	9.7*d*
RU Ophiuchi	28.1	+ 9 30	11.5*d*	RW Cygni	25.2	+39 39	8.2
RS Ophiuchi	44.8	− 6 40	11.4	RU Capricorni	26.7	−22 2	<12
T Draconis	54.8	+58 14	10.0	Z Delphini	28.1	+17 7	11.2 *i*
— Draconis	54.7	+58 14	11.2	ST Cygni	29.9	+54 38	11.8*d*
RY Herculis	55.4	+19 29	10.0	Y Delphini	36.9	+11 31	13.7
V Draconis	56.3	+54 53	11.8 *i*	S Delphini	38.5	+16 44	10.9*d*
T Herculis	18 5.3	+31 0	8.4 *i*	V Cygni	38.1	+47 47	11.8*d*
W Draconis	5.4	+65 56	11.0 *i*	Y Aquarii	39.2	− 5 12	11.2 *i*
X Draconis	6.8	+66 8	<12.6	U Capricorni	42.6	−15 9	12.1*d*
RY Ophiuchi	11.6	+ 3 40	11.2 *i*	T Delphini	40.7	+16 2	<13.7
W Lyrae	11.5	+36 38	12.0*d*	V Aquarii	41.8	+ 2 4	8.4
SV Herculis	22.3	+24 58	14.0*d*	W Aquarii	41.2	− 4 27	10.6*d*
T Serpentis	23.9	+ 6 14	9.8 *i*	V Delphini	43.2	+18 58	13.4*d*
RZ Herculis	32.7	+25 58	12.5*d*	T Aquarii	44.7	− 5 31	13.0*d*
X Ophiuchi	33.6	+ 8 44	8.8	RZ Cygni	48.5	+46 59	13.4*d*
RY Lyrae	41.2	+34 34	11.6	X Delphini	50.3	+17 16	10.6*d*
RW Lyrae	42.1	+43 32	11.4	UX Cygni	50.9	+30 2	11.2
RX Lyrae	50.4	+32 42	12.0 *i*	R Vulpeculae	59.9	+23 26	8.4*d*
ST Sagittarii	55.9	−12 54	14.0	V Capric.	21 1.8	−24 19	<12.5
Z Lyrae	56.0	+34 49	13.3*d*	TW Cygni	1.8	+29 0	11.4 *i*
RT Lyrae	57.8	+37 22	12.4*d*	X Capricorni	2.8	−21 45	11.0 *i*
R Aquilae	19 1.6	+ 8 5	6.4 *i*	X Cephei	3.6	+82 40	10.0 *i*
V Lyrae	5.2	+29 30	<13.6	RS Aquarii	5.8	− 4 27	12.4 *i*
RX Sagittarii	8.7	−18 59	13.2*d*	Z Capricorni	5.0	−16 35	11.6*d*
RW Sagittarii	8.1	−19 2	10.3*d*	R Equulei	8.4	+12 23	11.8*d*
S Lyrae	9.1	+25 50	<13.8	T Cephei	8.2	+68 5	8.4
RS Lyrae	9.3	+33 15	11.4 *i*	RR Aquarii	9.8	− 3 19	10.7*d*
RU Lyrae	9.1	+41 8	11.0	X Pegasi	16.3	+14 2	12.0 *i*
U Draconis	9.9	+67 7	13.5*d*	T Capricorni	16.5	−15 35	10.6 *i*
W Aquilae	10.0	− 7 13	12.9	Y Capricorni	28.9	−14 25	<13
T Sagittarii	10.5	−17 9	11.0*d*	S Cephei	36.5	+78 10	9.4*d*
RY Sagittarii	10.0	−33 42	7.3	RU Cygni	37.5	+53 52	8.3
R Sagittarii	10.8	−19 29	13.0*d*	RR Pegasi	40.0	+24 33	<13
S Sagittarii	13.6	−19 12	<13	V Pegasi	56.0	+ 5 38	10.4*d*
Z Sagittarii	13.8	−21 7	12.6*d*	U Aquarii	57.9	−17 6	11.2 *i*
TZ Cygni	13.4	+50 0	10.8*d*	RT Pegasi	59.8	+34 38	9.5 *i*
U Lyrae	16.6	+37 42	10.6	T Pegasi	22 4.0	+12 3	12.0 *i*
T Sagittae	17.2	+17 28	9.4	Y Pegasi	6.8	+13 52	10.4 *i*
TY Cygni	29.8	+28 6	12.6*d*	RS Pegasi	7.4	+14 4	<13
R Cygni	34.1	+49 58	10.6*d*	X Aquarii	13.2	−21 24	<11.6
RT Aquilae	33.3	+11 30	13.6*d*	RT Aquarii	17.7	−22 34	9.4
RV Aquilae	35.9	+ 9 42	<13.6	RV Pegasi	21.0	+29 58	9.0
RT Cygni	40.8	+48 32	10.8 *i*	S Lacertae	24.6	+39 48	<12.4
TU Cygni	43.3	+48 49	13.0*d*	R Lacertae	38.8	+41 51	<12.4
X Aquilae	46.5	+ 4 13	8.5 *i*	S Aquarii	51.8	−20 53	12.4
χ Cygni	46.7	+32 40	12.2*d*	RW Pegasi	59.2	+14 46	13.8*d*
RR Aquilae	52.4	− 2 11	12.6*d*	R Pegasi	23 1.6	+10 0	8.8*d*
RS Aquilae	53.7	− 8 9	11.4 *i*	V Cassiop.	7.4	+59 8	9.6 *i*
Z Cygni	58.6	+49 46	9.0 *i*	W Pegasi	14.8	+25 44	8.8
SY Aquilae	20 2.3	+12 39	12.2	S Pegasi	15.5	+ 8 22	12.4*d*
S Cygni	3.4	+57 42	11.8 *i*	R Aquarii	38.6	−15 50	10.0
R Capricorni	5.7	−14 34	10.8 *i*	ST Androm.	33.8	+35 13	9.2 *i*
S Aquilae	7.0	+15 19	10.0 *i*	Z Cassiop.	39.7	+56 2	13.2*d*
RU Aquilae	8.0	+12 42	13.5 *i*	RR Cassiop.	50.7	+53 8	<12.2
W Capricorni	8.6	−22 17	<13	Z Aquarii	47.1	−16 25	8.6
Z Aquilae	9.8	− 6 27	10.3	V Ceti	52.8	− 9 31	11.2 *i*
RS Cygni	9.8	+38 28	8.5	R Casssiop.	53.3	+50 50	8.0 *i*
R Delphini	10.1	+ 8 47	13.6*p*	Z Pegasi	55.0	+25 21	10.0 *i*
RT Capricorni	11.3	−21 38	8.0	Y Cassiop.	58.2	+55 7	<13
SX Cygni	11.6	+30 46	11.0*d*				

The letter *i* denotes that the light is increasing; the letter *d* that the light is decreasing; the sign <, that the variable is fainter than the appended magnitude.

The above magnitudes have been compiled by Mr. Leon Campbell of the Harvard College Observatory, from observations made at the Vassar, Mt Holyoke, Whitin, Swartz and Harvard Observatories.

New Variable Stars 139 and 140.1908.—In A.N. 4272 Prof. W. Ceraski announces two new variables discovered by Mme. L. Ceraski upon the Moscow photographic plates. Their places are as follows:

	α 1855	δ 1855	α 1900	δ 1900
	h m s	° ′ ″	h m s	° ′ ″
139.1908 Lyræ	18 30 19	+46 01	18 31 35	+46 03
140.1908 Sagittarii	18 41 05.6	−16 52 53	18 43 41.8	−16 50 07

The former varies from about 10^m to below $11^m.5$ and the period is probably long. The latter is B. D.—16° 5041 and on three photographs taken Sept. 27, 1899, July 23 and Aug. 20, 1906, appears of the same brightness, about $7^m.5$ while on a fourth plate obtained Sept. 18, 1906, it is about $0^m.8$ fainter.

New Variable 141.1908 Draconis.—This is announced by Prof. W. Ceraski in A.N. 4277. From 22 photographs taken at Moscow during the years 1904–1908 the range of variation appears to be between the 10th and 12th magnitudes and the period perhaps about twelve months. Its position is

1855	$\alpha = 17^h\ 30^m\ 53^s$	$\delta = +54°\ 02'$
1900	17 31 49	+54 00

New Variable 142.1908 Cassiopeiæ.—In A.N. 4277 Mr. Sigurd Enebo calls attention to a star near the variable SX (142.1907) Cassiopeiæ, which during the winter of 1907–08 must have been below magnitude 10.5. On Aug. 5, 7, 9, 13 and 15 last it was not noticed during the observations of SX, but on Aug. 20 it was estimated at $9^m.4$ and on Sept. 8 at $9^m.6$. Since that time it has been reckoned as below $10^m.0$ Its color on Aug. 20 was strong red. Its position is

1855	$\alpha = 0^h\ 02^m\ 36^s$	$\delta = +54°\ 04'\ 24''$
1900	0 04 55	+54 19 26

On Oct. 15 its brightness was estimated at about $11^m.0$. The star is the third of four faint stars which follow each other in a straight line, only a few seconds apart in right ascension.

Algol.—In A.N. 4278 Mr. Charles Nordmann gives the results of his observations of six minima of β Persei (Algol), from which he concludes that the ephemerides of Algol in "Annuaire du Bureau des Longitudes" require a correction of about -53^m. These ephemerides agree with those published in POPULAR ASTRONOMY so that the minima of Algol may be expected to occur about an hour earlier than predicted in our tables.

Antalgol Variable ST Virginis.—In A.N. 4277 Dr. E. Hartwig gives as elements of this variable

$$\text{Max.} = 2418121.4146 + 0.^d41139.$$

It is of the antalgol type remaining at minimum about five hours and at maxi-

mum only ten minutes. The range of variation is from about 10.5 to 9.1 magnitude. The place of the star is

1855	$\alpha = 14^h\ 20^m\ 12^s.44$	$\delta = -0°\ 14'\ 52''.1$
1900	14 22 30.98	−0 27 07.4

Sixteen New Variables in the Harvard Map, Nos. 4 and 13.—In the Harvard College Observatory Circular No. 140 Prof. Pickering gives a list of sixteen new variables discovered by Miss Cannon in examining the photographic plates covering the region of the Harvard Map, Nos. 4 and 13. In A.N. 4275 these are given the numbers 44 to 59.1908.

Provisional Designation	Harvard Number	BD °	R.A. 1900 h m s	Decl. 1900 ° ′	Mag.	
44.1908 Aurigæ	3086	+39 1225	5 08 14	+40 01.0	9.3	10.1
45.1908 Aurigæ	3087	. . .	5 35 02	+38 53.2	9 5	<12
46.1908 Tauri	3088	+13 971	5 39 23	+13 31.9	8.5	9.4
47.1908 Tauri	3089	. . .	5 43 12	+19 02.0	10.0	<15
48.1908 Tauri	3090	+28 921	5 45 49	+28 05.2	9.4	11.0
49.1908 Aurigæ	3091	+45 1202	5 49 42	+45 29.1	9.3	10.3
50.1908 Orionis	3092	+13 1034	5 50 11	+13 40.2	9.7	10.7
51.1908 Geminorum	3093	+24 1056	5 54 33	+24 28.1	9.3	10.3
52.1908 Geminorum	3094	+22 1146	5 56 35	+22 14.7	9.5	11.0
53.1908 Orionis	3095	. . .	5 57 16	+16 22.3	9.5	<12
54.1908 Geminorum	3096	+11 1187	6 02 32	+22 37.9	9.0	10.0
55.1908 Lyncis	3097	+61 887	6 20 24	+61 37.1	9.2	9.9
56.1908 Geminorum	3098	. . .	6 22	+20 37	9.6	10.5
57.1908 Camelop.	3099	. . .	6 23 40	+67 06.0	10.6	<11.6
58.1908 Camelop.	3100	. . .	7 04 09	+73 29.7	9.8	10.9
59.1908 Ursæ Maj.	3101	+65 613	8 01 41	+65 31.6	9.2	10.1

Notes.—44.1908 period probably short. 45.1908 Maxima = J. D. 2416791 + 452 E. 47.1908 variation resembles that of R Coronae Borealis. 48.1908 period short. 49.1908 nature of light curve not indicated. 50.1908 Algol type. 51.1908 Algol type? 52.1908 period short. 53.1908 period probably long. 54.1908 period probably short. 55.1908 character of light curve not indicated. 56.1908 probably Algol type. 57.1908 period unknown. 58.1908 probable Algol type. 59.1908 type unknown.

Variable Star Note.—From an observation by Leon Campbell with the 24-inch reflector on October $19^d.619$, G. M. T. the variable star 31.1907,—Aurigae was found to be bright, magn. 10.8.

The variable star SS Cygni, 213843, has been undergoing remarkable changes during the past month. On Sept. 15 it had reached a maximum of magn. 8.3, of the type known as Anomalous. On Oct. 6 it had decreased to magn. 11.0, and on Oct. 12 it was increasing in brightness and of the magnitude 10.5; while on Oct. 19 it appears to have attained the magn. 10.0. In other words, the variable had not reached its usual minimum magnitude, 11.8, before it started to increase again.

EDWARD C. PICKERING.

Astronomical Bulletin No. 341,
Harvard College Observatory, October 20, 1908.

GENERAL NOTES.

A Central Eclipse of the Sun will occur on December 23, but will be invisible in the Northern Hemisphere. It will be annular at the beginning and end and will be total in the middle. The path of the central line of eclipse will pass across South America, crossing Chili, Argentine Republic and Uraguay, perhaps touching a corner of Brazil. Thence its course will be wholly over the waters of the southern Atlantic and Indian Oceans. The duration of totality will be only eleven seconds at the most. As a partial eclipse the beginning may be observed all along the southeast coast of South America from the mouth of the Amazon down, in the southern point of Africa and the Island of Madagascar. For the elements of the eclipse see POPULAR ASTRONOMY No. 151, January 1908, page 48.

Photographic Observations of Comet Morehouse. Beginning with the night of October second Comet Morehouse has been photographed here on every clear evening; these photographs have been made with a photographic doublet of nine inches aperture, with a ratio of aperture to focal length of 1 to 5, which has been recently installed in the observatory here.

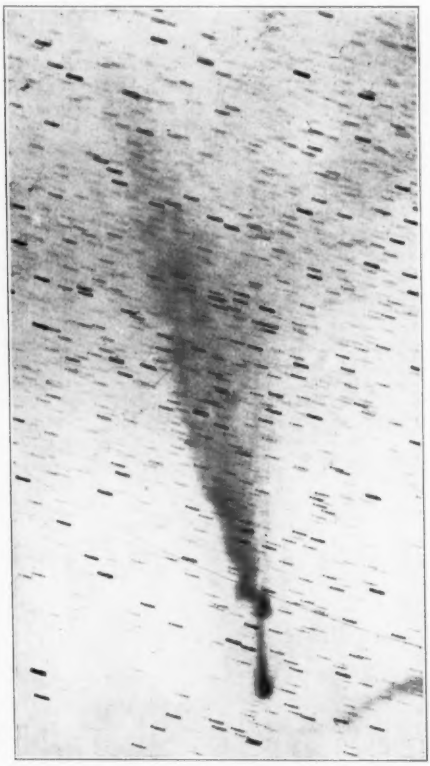
The instrument was built by the John A. Brashear Company and is a part of the astronomical equipment presented by William C. Sprove to Swarthmore College.

This comet has been whimsical in its behavior, so much so, that, one could not predict with any degree of confidence what, on any one night, the shape of its tail might prove to be.

A most striking change occurred during the twenty-four hours beginning at eight o'clock Washington mean time October fourteenth. A photograph made on the night of the fourteenth showed the comet with a quasi fan-shaped tail. It was not symmetrical, but was much brighter on one side, and there issued from the head and seemingly from various parts of the tail long, thin, straight, and nearly parallel rays. The appearance of the tail on the night of October 15 is well shown in a photograph, the negative of which I exposed from 7:00 to 8:30 o'clock Washington Mean time. I exposed a second plate on the same night from 9:20 to 10:20. Both plates showed the straight narrow tail joined to the head of the comet as well as the long broad irregular ribbon-like tail the beginning of which was at some distance from the head of the comet. The first negative, which was exposed longer and which was made when the altitude of the comet was greater showed thin straight rays apparently issuing from points in the comet's tail, resembling somewhat those of the night before. A comparison of the two plates showed that the beginning of the tail (*i. e.* the part nearest the head of the comet) was moving faster among the stars than the straight tail that was joined to the head of the comet.

At the beginning of the broad tail, I selected two condensations, the one on that side of the tail which was in the direction in which the comet was moving, I called condensation one, and the other at nearly the same distance from the head but on the opposite side of the tail I called condensation two. These condensations are easily identified on both plates. Both were farther from the head on the second plate than on the first, the first having moved a distance of 5′ 45″.5 of arc and the second a distance of 5′ 34″.

On the night of the sixteenth Mr. Walter R. Marriott exposed a plate for one and one half hours beginning at seven thirty. The condensations were found on this plate, though owing to the fact that they had changed their shape somewhat they could not be so certainly identified as on the preceding night. Condensation one on this plate was 1° 17′ 47″ farther from the head than it was on the first plate exposed on the fifteenth and condensation two had moved in the same time through an arc of 1° 16′.



COMET MOREHOUSE.
October 15.

No traces of the condensations, nor of any part of the broad ribbon-like tail could be found on a plate exposed on the seventeenth.

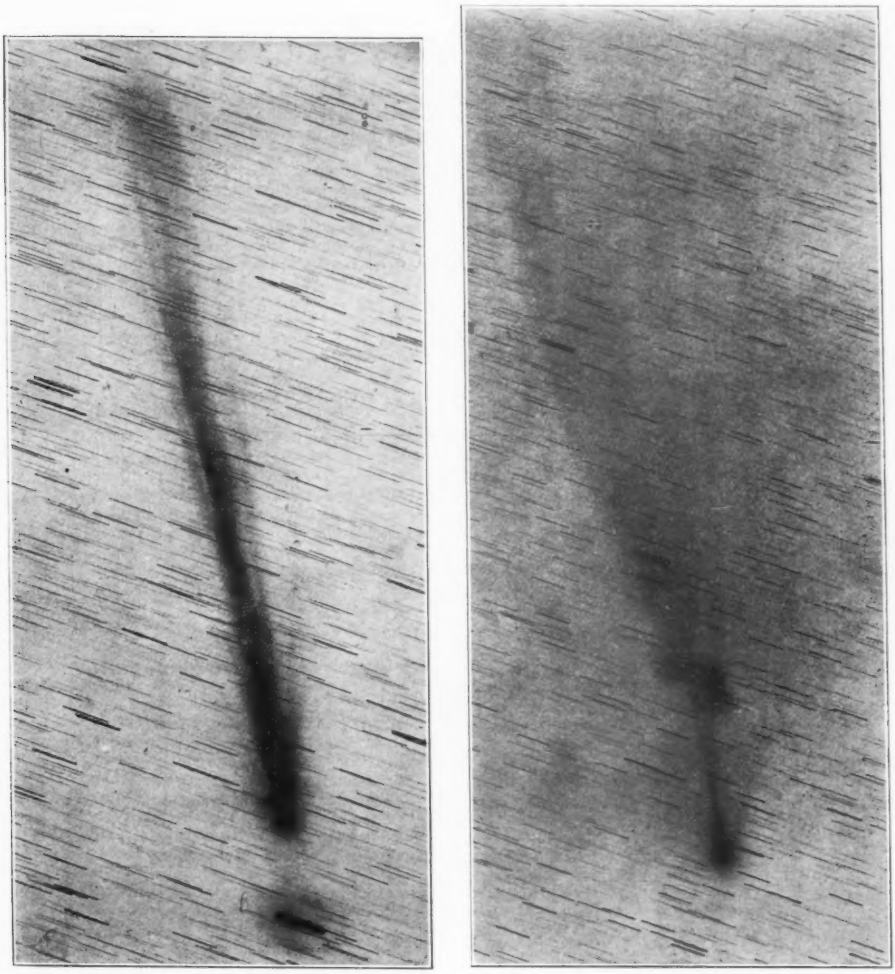
If we assume that the tail of the comet is in the direction of the radius vector of the comet from the Sun, condensation one had receded from the head of the comet, 224,000 miles, and condensation two about 247,000 miles in two and one half hours on the night of the fifteenth. This requires an average velocity of 89,600 and 99,000 miles per hour respectively.

A comparsion of the first plate of the fifteenth with that of the sixteenth

shows that the average velocity of the two condensations for 24½ hours was 142,000 and 129,000 miles per hour respectively.

It is probable that the tail of the comet deviates enough from the radius vector, to modify slightly these velocities.

Swarthmore, Pa. JOHN A. MILLER.



No. 1, Oct. 14, '08, exposed $7^h\ 44^m$ to $9^h\ 46^m$ E.S.T. No. 2, Oct. 15, '08, exposed $8^h\ 0^m$ to $9^h\ 30^m$ E.S.T.
COMET MOREHOUSE PHOTOGRAPHED BY REV. JOEL H. METCALF.

The Pope and the Comet. *A Reply to Mr. Dean.*

1. The first sentence in my article is *dis*junctive, not *con*junctive, writers on Halley's comet bring in the bull OR the bells OR the prayers. When Mr. Dean pleads guilty to the third, I do not therefore commit him for the first and second.

2. Yes, my article was inspired and hastened by his. I judged him to belong to that large class of readers who see one or two or all three of these statements about Callixtus III almost everywhere in magazines and newspapers, and never hear them contradicted, and therefore never even suspect them to be false, and then propagate them bona fide.

3. Mr. Dean will find a previous similar allusion to this comet story on page 239 of No. 144 of this journal, April 1907. As the article in which this allusion occurs, was reprinted from *Knowledge* and *Scientific News*, and later also appeared in the *Scientific American*, and as similar articles have also been copied by our local Sunday newspapers, I had good reason to conclude that the error had already crept into many other publications, and would keep on spreading unless it was checked by a timely article in a magazine as important as POPULAR ASTRONOMY.

4. That Callixtus III ordered the bells to be rung at noon or introduced the prayer called the Nagelus, is not at all as certain as writers would have us believe. If he did, neither the bells nor the prayer had any connection with the comet, and that is the point at issue. Nor did he add the comet invocation to the litanies or the Angelus. In one word, no historical proof has ever been advanced which would connect Callixtus III with Halley's Comet.

5. Present day writers rarely mention the bull. When they do, they always prefix the phrase "it is said." But many former writers really did believe that the pope issued a formal bull against the comet, and that he ordered bells to be rung to frighten it away. Mr. Dean will find in Fr. Gerard's article (mentioned by me on page 482 in my article) quotations and references which I do not wish to perpetuate.

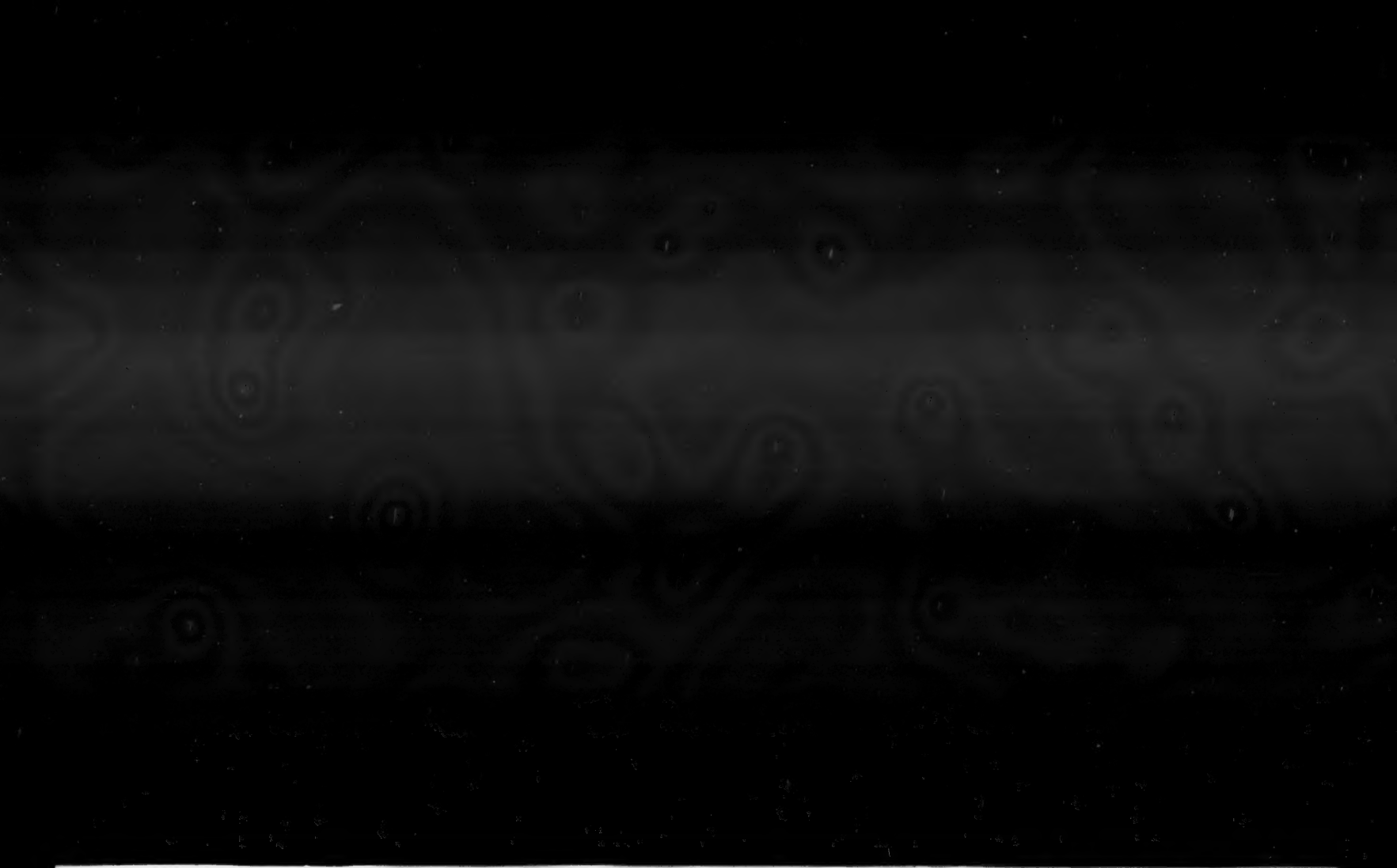
Creighton University,
Observatory, Omaha, Nebraska. WILLIAM F. RIGGE.

Photographic Films. For some time past European astronomers have been interested in the study of photographic films to obtain knowledge of their elementary structure. The object of this study has been to learn, if possible, whether or not the discordances in the measures of photographic plates in determining exact star places were due to something else than mere errors of measurement.

C. D. Perrine has given in No. 143 of the Lick Observatory Bulletin, some results of a study of the grain and structure of photographic films the summary of which we give as follows:

"The conclusion drawn this investigation is that practically all the large discordances found in the positions of the stars derived from photographs can be traced to the *structure* of the star image itself due to vacant lanes and other irregular arrangements of the silver grains.

It is earnestly hoped that the manufacturers of photographic plates, and others who may have the necessary facilities, will make a strong effort to produce a more uniform structure of sensitive films suitable for original exposures. At the present time I am inclined to believe that such a result is even of greater importance than increased sensitiveness. Could the size of the grain be reduced also, an improvement in positions could hardly fail to result to say nothing of the manifold other ways in which scientific photography could be helped.

It is encouraging to find such strong evidence that practically all the trouble is in one place, and particularly that the telescope, seeing, etc., appear to introduce but slight errors.

It may not be out of place to emphasize the experience of many astronomers that fair sized black images of stars usually give better positions than can be obtained from very small, faint ones."